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*Collaborators* — Dr. Edward W. Taylor, Instructor in Neurology, Harvard Medical School, Dr. George A. Waterman, Instructor in Neurology, Harvard Medical School, Dr. E. B. Holt, Dr. J. C. Bell, and Dr. Robert M. Yerkes, of the Psychological Department, Harvard University, Dr. J. W. Courtney and Dr. Isador H. Coriat, of the Neurological Department, Boston City Hospital, and Dr. August Hoch will aid in the department of current literature, which will include neurology, normal and comparative psychology, as well as abnormal psychology and psychiatry.

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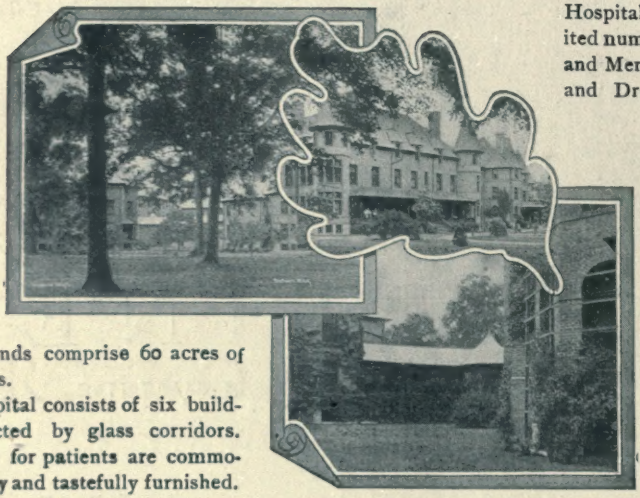

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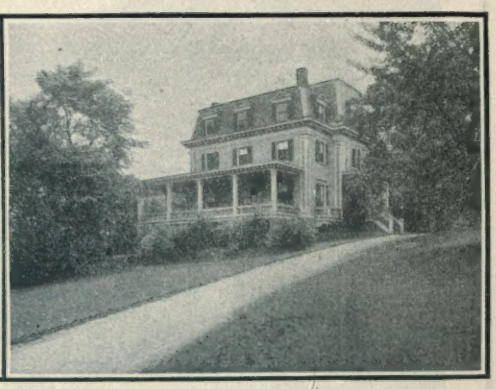

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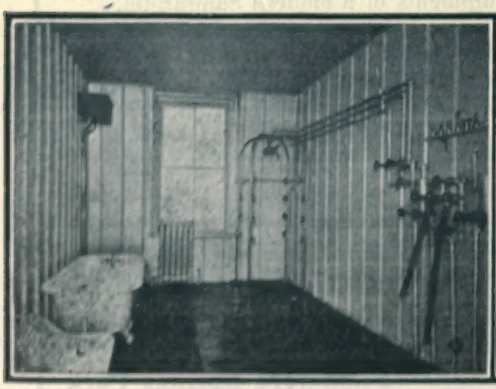

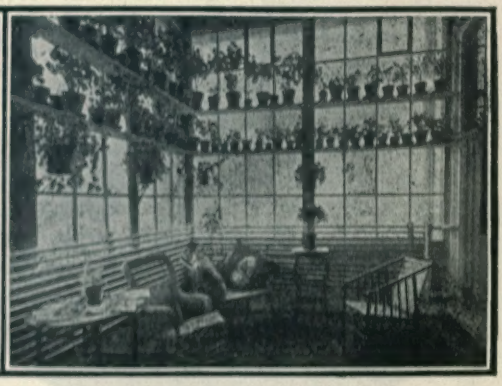

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THE JOURNAL OF
ABNORMAL PSYCHOLOGY

APRIL–MAY, 1908

# AN EXPERIMENTAL STUDY OF SLEEP

BY BORIS SIDIS, A.M., PH.D., M.D., BROOKLINE, MASS.

*"Hypotheses non Fingo." — Newton*

## PART I

### CHAPTER I

### *Introductory Remarks*

WRITERS on sleep complain that little attention has been paid to the subject, that it is sufficient to open a text-book on physiology to be convinced of the fact that the physiology of sleep is almost entirely neglected, inasmuch as the school-physiologist usually dismisses the subject with a few phrases, often quite general and devoid of meaning. It is quite true that while one cannot as a rule be satisfied with the imperfect state of textbook-physiology which usually lingers in the hind ranks of the battle-ground of science, still one cannot blame the text-books for avoiding such a delicate subject the nature of which is so uncertain and so highly problematic.

In addition to the uncertainty of the subject of which the more conservative of school-physiologists fight shy, there seems to be an ill defined feeling which is not without some good foundation, namely, that sleep is not entirely a physiological subject, that sleep presents some very important aspects that need be taken into consideration which the

phys ologist is unable to deal with by his usual methods and rom his standpoint alone.

Moreover there may be another reason for the indifference of school-physiology to a subject which is otherwise of such a vital importance in the whole domain of animal life. Man is more interested in active than in passive states. It is therefore natural that the physiologist should devote his attention more to waking life than to sleeping states. Besides, physiology dealing essentially with activities and functions tends to ignore states which are usually regarded as the very acme of inactivity. With the advance however of biological, physiological and psychological sciences even states of passivity can no longer be ignored,— their onditions, causation and nature must be studied and closely investigated, especially if those states are found present throughout the ascending line of animal life. The conditions for the study of sleep become all the more favorable as we reach man. We find that sleep-states in man's life are no longer instable and taken by snatches, because the watchfulness requisite in wild-life under the constant strain of the struggle for existence no longer obtains. In man's life sleep-states become more or less organized, systematized and are no longer disturbed,— they alternate rhythmically with waking states. More than one-third of man's life is passed in sleep,— it seems that this fact alone should indicate the importance of sleep-states in man's cycle of biological and physiological processes and should arouse the interest of the scientific investigator. As a matter of fact some scientific thinkers have given the subject of sleep a good deal of their attention. Although text-book physiology passes over the subject with a few meaningless phrases, the literature of sleep is really very extensive. It may be well before we proceed with the exposition of our own observations and experiments on sleep to give first a brief review of the chief theories on sleep.

## Chapter II

### *Theories of Sleep*

FROM the very earliest times man wondered about sleep and attempted some explanation of it. It was supposed that in sleep the soul wanders away from the body and leaves it in a lifeless condition. Many of the savage tribes are on that account afraid to waken people lest the soul may be frightened away and not return at all to the body. That is why even at present we often hear the saying that sleep is the companion of death. On the other hand death is often described as sleep. On the same basis dreams were explained by the primitive mind,— the soul in sleep leaves the body and wanders away; on its wanderings the soul meets with all kinds of adventures and it is such experience that gives rise to dreams. In sleep the wandering soul, not encumbered by the gross body, can visit great distances in a short time and can even communicate with heavenly powers,— with angels and gods.

The impassive soul reluctant flies,
Like a vain dream, to these infernal skies.

Modern spiritualists adopt this ancient belief in full or express it as "the release of human personality from the subliminal."

The scientific theories of sleep are numerous, but they can be reduced to a few main types. This reduction will help the reader to become oriented in the vast literature that has gathered about the subject. The theories of sleep may be classified as follows:

1) The Physiological theories, which may be subdivided into:
   a) Mechanical
      and
   b) Chemical
2) Pathological
3) Histological
4) Psychological
5) Biological

The mechanical theories are of the circulatory variety and are usually much favored by physicians,— they attempt to explain sleep by changes in the blood circulation of the cerebrum. As far back as the eighteenth century the theory of sleep greatly in vogue among the physiologists was — congestion of the brain favored by the position of the head. This view was entertained by Haller, J. Müller, Hartley and others. Cappie has somewhat enlarged on it, inasmuch as he ascribes sleep to the venous congestion of the brain. This congestion of blood was supposed to bring about pressure on the brain with the result of depression of all the cerebral functions. The great psychologist Hartley summarises the physiological theories of his time thus: "It appears then that during sleep the blood is accumulated in the veins and particularly in the venal sinuses which surround the brain and spinal marrow. . . . And it is agreeable to this that in most dissections after lethargies, apoplexies, etc., the venal sinuses of the brain and consequently those of the spinal marrow which communicate freely with them are particularly full. . . . It follows therefore that the brain and spinal marrow will be particularly compressed during sleep, since the blood then takes up more space, is particularly accumulated within the cavities of the skull and vertebrae, and the hardness of these bones will not suffer them to yield or make more room. . . . In short this compression will result in sleep." These generalizations were favored by observations on the famous case of the Parisian beggar whose injured skull gave the opportunity to observe the rise and fall of the exposed cerebrum in the waking or sleeping state. Recently Mosso claimed the same causation of sleep and on similar observations. As to the circulatory changes proper, two opposing views have been taken. Some, such as Durham, Cl. Bernard, Kussmaul Howell, Lehman, de Fleury ascribe sleep to cerebral anemia. Others again, such as Brown, Czerny, Broadman maintain that sleep is due to cerebral hyperemia. Claperede in reviewing the circulatory theories of sleep quotes Richet's apt criticism: "Sleep and waking states bring about all kinds of changes in cerebral circulation depending but little on the position of the head. Birds whose hemispheres have

been removed still present the same changes of sleep and waking. Biologically regarded, sleep is a far more general phenomenon than that of the presence of a brain irrigated by a blood circulation. Finally the activity of a tissue is not entirely dependent on the amount of blood circulating in it." There are however still more cogent objections to such theories of sleep, objections which clearly show the incongruence of the circulatory theories. We shall point them out further in our present study.

We may turn now to the chemical theories which are far more favored by the conservative physiologist, inasmuch as they fall in with what the physiologist regards as more scientific. With the experiments of Pettenkofer and Voit on the respiratory quotient new life was injected into sleep theories. Those two investigators have found the respiratory quotient $\frac{CO_2}{O}$ is diminished during sleep. The tissues absorb relatively more oxygen during the day than during the night. This fact of the using up and impoverishment of blood of intermolecular oxygen started new life in the theories of sleep. Pflüger, Sommer and others attempted to work out a scientific theory of sleep based on research of the biochemistry of the cell and the intermolecular activity of the oxygen-molecule or atom. Pflüger's authority lent vitality to this view so that even Heubel whose theory of sleep is really psychological in character tries to shelter it under Pflüger's physiological wings.[1]

With the chemical theories we pass by degrees into the pathological theories of sleep. Already in the early part of the nineteenth century Marshall Hall proposed the view that sleep was a kind of epilepsy. This view however met with little favor, because of lack of facts to support it. With the advance of chemistry and of its application to physiological research, and especially with the rise of the modern views of the rôle of autointoxication and toxins in diseases, the pathological theories of sleep were resuscitated under the new guise of autointoxication. Obersteiner and Preyer launched the theory that sleep was an autointoxication of the system by toxic matters accumulating in the blood, due to the

[1] We may possibly refer to Loeb's work on heliotropism and sleep of butterflies as an example of a purely bio-chemical view of sleep. See Dec. Public Univ., Chicago, V. I.

activity of the various tissues. Lactic acid was supposed to be the particular substance in question. Others such as Dubois ascribed the same state to the accumulation of carbonic acid. These were followed by a host of writers such as Binz, Errero, Bouchard, Breisacker and others. The toxic and autotoxic theories of sleep enjoy quite a wide popularity, because they fall in with the scientific notions of the age. Those theories would make of sleep a pathological state, but the facts are against such a view.

By the middle of the nineteenth century, when physiological experiments on nerve-conduction were at their height, Purkinje proposed the theory that sleep might be due to the interruption of neural conductivity between the cortical matter and the rest of the cerebrum. This view was further developed by Mautner, Warlomont, Oppenheimer and others. As a further modification of the same theory, but based on more fundamental physiological processes may be mentioned Verworn's theory which refers sleep and waking-states to processes of assimilation and dissimilation going on in the organism. At the same time with Verworn, Van Gieson and Sidis worked out their theory of sleep, basing it on similar processes, namely on anabolism and catabolism and developing the interrelation of waking and sleeping states with the symptom complexes of nervous and mental diseases. The theory of Van Gieson and Sidis is based on the variability of different levels of neuron energy. We may dismiss this latter theory in a few words as we shall discuss it in our study. We can only say here that the theory is essentially based on the concept of neuron-energy.

This brings us close to the famous theories of retractility of neuron-elements which have been of late utilized by many writers for various purposes. The development of the histology of the nervous system and especially of neuropathology have brought new life to the solution of the problem of sleep. The biological investigations of the cell by Kölliker, Remak, Nageli, Hoffmeister, Virchow, Max Schultze, Hertwig, Fol, Van Beneden, Strassburger, Heidenhein, Boveri and many others opened up new horizons for theories of cellular activities. Naturally the sleep theories

came in for their share. It was not however until the researches of Golgi and Ramon Y Cajal had laid the foundation for their famous doctrine of independence and contiguity of neural elements that the theories of sleep could seize on some tangible anatomical facts and work them for their benefit. Ramon Y Cajal was the first to advance the view that the neuroglia-cells by their expansions and contractions bring about dissociations and associations of the neural elements with the consequent loss or reappearance of normal waking-states. A somewhat different, but closely analogous theory was launched into the scientific world by Mathias-Duval. Instead of the retractility of neuroglia cells proposed by Cajal, Duval advanced the view that the neurons and their protoplasmic processes are endowed with contractility or retractility and that the functioning and loss of functioning of neural elements are due to contractions and expansions of the protoplasmic processes of the neurons. Demoor, Pupin, Berger and others have enlarged on this hypothesis. At first even Verworn accepted this hypothesis, although he discountenanced it afterwards. Sidis assumed provisionally the same hypothesis for abnormal dissociative manifestations, but refused to accept it as an explanation for the phenomena of sleep which do not warrant the assumption of such hypothesis.

The psychological theories of sleep date from antiquity. The popular explanation is very simple,— it is a description of the phenomena, a description which the popular mind often takes for an explanation of the phenomena in question. Sleep is an abeyance of mental life, sleep is a rest of consciousness. Modern physiologists and psychologists who maintain the psychological theory of sleep have not much improved on that statement. The only modern substitute is that of inhibition. Brown-Sequard, Wundt, Siemens, Forel, Oscar Vogt, all in different scientific phraseology refer sleep to inhibition of cerebral activity, especially of the frontal lobes where mental activity is supposed to "reside," according to some authors. Manaceine in her work on sleep echoes this view and comes to the conclusion that "sleep is the resting state of consciousness." Surely it is an elaboration of the obvious, if one has to write a whole volume

in order to arrive at such an important conclusion. The only one who really made some advance in the psychological theory of sleep was Heubel, Privatdocent at Kiev University, Russia. I can say his is an excellent piece of work based on a series of well performed experiments. Unfortunately, when I started my experiments, I was not acquainted with the work of that investigator. I have gone over in my experimental research a good deal of the same work and may say that Heubel's experiments have been fully confirmed by me. Although Heubel's theory is incomplete, there is a good deal of truth in it and his work well deserves the attention of the student of sleep. His veiw may be briefly summarized in the following statement: Mental activity depends on the incoming peripheral, sensory stimulations; where such peripheral sensory stimulations are absent, mental activity is in abeyance and sleep results. In other words, brain activity depends on sensory activity which in its turn depends on peripheral stimulations. Psychologically stated, consciousness is a function of sensations which in their turn are a function of external stimuli or impressions. In accordance with this view a series of experiments have been carefully carried out by Huebel. As my own experiments were carried out on somewhat similar lines I shall refer again to Huebel's work, when I give an account of my experiments, in order to test a somewhat similar theory arrived at from totally different considerations, viewing the subject of sleep not only from a physiological and psychological, but also from a biological standpoint.

An altogether different departure from the usual theories of sleep was recently taken by Claperède. He points out that biologically regarded, sleep has its significance not as a passive state, but as an active instinct, like all the other instincts of animal life. To put it in his own words: "Le sommeil n'est pas un état purement négatif, passif, il n'est pas la consequence d'un simple arrêt de fonctionnement: il est un fonction positive, un acte de ordre rèflexe, un instinct, qui a pour but cet arrêt de fonctionnement; ce n'est pas parce que nous sommes intoxiqués, ou épuises, que nous dormons, mais nous dormons pour ne pas l'être." This biological view of Claperède forms one of the most

valuable contributions to the theory of sleep. It throws altogether new light on the subject of sleep, and many obscure until now unexplained facts can be understood in the new light of the biological standpoint taken by Claperède. Claperède's physiological theory of inhibition is not as clear, but with some modification I think it could well harmonize with my views and my work on sleep. While accepting the views of Claperède, my standpoint taken in this study is based on investigations published in previous works of mine mainly dealing with the subject of dissociative states in general.

## Chapter III

### *The Conditions of Sleep*

TAKING the well-known dictum of Newton, "Hypotheses non fingo," let us start with facts before we attempt to form any idea as to the nature and character of that apparently mysterious state known as sleep. In order to understand a highly complicated phenomenon it is well to study the circumstances under which it occurs and investigate the conditions that favor the manifestation of the state. Now in studying the conditions of normal and abnormal suggestibility, I pointed out in my "Psychology of Suggestion" that the following conditions are requisite to bring about those peculiar subconscious or subwaking states which form the soil favorable for the growth and development of implanted suggestions.

| Conditions of Normal Suggestibility | Abnormal Suggestibility |
|---|---|
| 1) Fixation of attention | 1) Fixation of attention |
| 2) Distraction of attention | 2) |
| 3) Monotony | 3) Monotony |
| 4) Limitation of voluntary movements | 4) Limitation of voluntary movements |
| 5) Limitation of consciousness | 5) Limitation of consciousness |
| 6) Inhibition | 6) Inhibition |
| 7) Immediate execution of the suggestion | 7) |

I have further pointed out that what we really bring about under such conditions is a dissociation of consciousness. In the normal waking state the dissociation is transient, fleeting, disappearing at the very moment of its appearance, while in abnormal suggestibility the states are more or less permanent. In other words, the waking state tends to disappear under the conditions mentioned above. Now to induce sleep the first requirement is the displacement of the waking state. It is evident that the conditions that favor the suppression of the waking state would also favor sleep-states. As a matter of fact I have often observed in my patients and subjects that the conditions by which I intended to bring about a subconscious state in general and a hypnotic state in particular have often resulted in ordinary sleep. I have found quite frequently that the close observation of the conditions of monotony, limitation of the voluntary movements, limitation of the field of consciousness and of inhibition, brought about not a hypnotic state, but ordinary sleep. I began to experiment on myself and found that I could put myself almost at any time into a state of deep sleep by closing my eyes and keeping perfectly still, dismissing all ideas from my mind — closing shop, so to say. I was able to put myself into a quiet, prolonged and often very refreshing sleep. I have further succeeded in the treatment of many cases of insomnia, by following the same lines in bringing about good therapeutic results.

After many years of experimentation and observation I have come to the conclusion that this could not possibly be an accidental matter, but that there must be some close interrelation between sleep-states and the conditions of normal and abnormal suggestibility. In other words, the conviction was almost forced on my mind by the facts that the conditions I found requisite for the induction of dissociated states and for normal and abnormal suggestibility in general play also an important rôle in the induction of sleep.

There is one fact that is specially worth while noticing in the process of falling asleep, and that is, the circumstance that we do not tumble into it in spite of ourselves, that we go about it in a deliberate, may be cold-blooded fashion.

We have to make up our minds and decide whether or no we want to go to sleep, or whether it is best for us to be asleep; and when we decide in the affirmative, we undress and go to sleep. The whole affair is not the after-effect of some narcotic, toxic, autotoxic bodies, nor the result of some involuntary automatic mechanism,— it is a voluntary act,— the result of decision. In this respect sleep is very similar to the abnormal state of suggestibility in which subjects can be artificially put. The state of abnormal suggestibility, the hypnotic state, is not induced by accumulation of toxic products in the system. The state can be brought about at will under the conditions described above. It is a purely voluntary affair, requiring the coöperation of the subject's and the experimenter's attention, active and steady. In fact, subjects whose attention is poor,— imbeciles, idiots, insane persons or persons whose attention is constantly wandering and fluctuating, are extremely difficult to hypnotize. These significant facts so often overlooked by writers on the subject, force on one the conclusion that the psychic factor is of the utmost importance in the formation of sleep-states.

If we now scrutinize the conditions under which drowsiness and sleep occur, we find that they differ in some very important points from those requisite in the induction of hypnotic states. In the bringing about of hypnosis the most favorable condition is the fixation of the attention on some object, perceptual or ideational. So much is this condition of fixation of importance that some writers describe the nature of the subconscious by this one condition of the attention, and characterize the hypnotic state, not without some show of plausibility, as a cramp of the attention. Such a sweeping statement is not entirely true to fact, but it is true that fixation of the attention, steady and persistent, without flinching, constitutes a very important factor in the induction of the hypnotic state. In sleep, however, we observe that the condition is somewhat modified,— it is not fixation of the attention that is conducive to sleep; in fact, fixation of the attention is more favorable to insomnia or to the prolongation of the waking state. What is requisite is a *relaxation* of the attention. If fixation of the attention is present at all, it is only present in so far as it is requisite

to have the attention relaxed, which under some conditions requires either an effort of the will or the presence of specially favorable circumstances. It is the will to give up all active relations, sensory and motor, with the external environment. In hypnosis there is present at first a strained state of attention; in the induction of sleep the strain is practically reduced to its minimum. In the formation of sleep-states all active desire must cease. That is why people with intense, active desires and emotions find it so difficult to fall asleep. On the other hand such intense desires and strong emotional states may, if taken advantage of at a favorable moment, become the best condition for bringing about states of suggestibility, hypnosis and all forms of dissociation. In fact, in this respect we may say that the sleep-states differ fundamentally from hypnotic states. Suggestibility, which is the characteristic trait of subconscious states and of hypnotic states in particular, is absent in sleep-states. While hypnosis and allied states are characterized by a greater facility of reactions to external stimulations, the sleep-states on the contrary are characterized by an almost complete suppression of the more complex reactions associated with mental processes. In other words, in hypnosis and allied states there is a suppression or inhibition of the inhibitions present in waking life; in sleep, on the contrary, the inhibitions are intensified. If put in terms of the threshold theory advanced in my work, "Multiple Personality," we may say that in hypnosis and allied states the thresholds are lowered, while in sleep states the thresholds are raised.

If we turn once more to the conditions of sleep, we find that limitation of the field of consciousness and limitation of voluntary movements play a very important rôle in the bringing about of sleep-states. In this respect Huebel's shrewd observation is correct,— the cutting off of all external stimulations tends to bring about a languid condition akin to sleep. Prisoners in solitary confinement, unless they find a source of mental activity by getting some external stimulations to awaken their mental life, have a tendency to sleepiness. Where attention is relaxed, or the interest in external impressions is gone, there drowsiness supervenes.

In the limitation of voluntary movements a mass of muscular sensations as well as kinaesthetic sensations is kept from pouring into consciousness, the result is a lowering of mental activity, a rise of thresholds characteristic of sleep. The cutting off of external impressions is also of importance in hypnosis, as when the subject is asked to keep very quiet and make his mind a "blank." Where there is a predisposition to states of dissociation the result of limitation is not sleep but hypnosis, with a fall of thresholds.

The most important and possibly fundamental condition common to sleep and subconscious states is that of *monotony*. In order to fall asleep we must dismiss all our interests, all our thoughts. Similarly, to induce hypnosis we tell the subject to try not to think of anything. We impoverish, we make *monotonous*, his mental life by making him think of "nothing in particular."

The fact that people may fall asleep even under intense stimulations is often adduced as a strong objection to the view that diminution of sensory stimulations is conducive to sleep. Thus Richet refers to the fact of "falling asleep at the opera, in spite of the light and the noise." This objection is valid only if we leave out of account the importance of the factor of monotony. It is not so much the diminution of the intensity of stimulations which is of importance in the production of sleep, as the total mass of impressions. In fact, it is not so much the total volume as the progressive, ceaseless variability of the incoming impressions that counts in the keeping up of the restless activity of consciousness in its adjustment to the external conditions of the environment. Given a prolonged stimulation or a series of stimulations of the same intensity, consciousness becomes dulled and sleep ensues. In fact, the more intense the monotonous stimulations are, the deeper is the state of sleep.

It is interesting to note that under such conditions the intensity of the series of monotonous stimulations tends to keep up the sleep-state. With the cessation of the monotonous stimulations the sleep-state tends to disappear. The miller falls asleep under the continuous uniform noise of his mill, and wakes with the cessation of the noise. In

listening to lecturers in a medical school I found how easily the monotonous, uninteresting delivery put me into a drowsy state, and how I came back to myself with a start when the lecturer stopped. Inquiring among the students, I found that their experiences were quite similar to my own; they were kept in a semi-drowsy state by the long-drawn-out sentences, and kept awake by the lecturer's resting-places. Many people who are used to the long, continuous hum of a large city such as New York, for instance, find it difficult to fall asleep in a quiet place.

## Chapter IV

### *Intermediary States*

In many cases where it is requisite to find the causation of the mental trouble, and hypnotization is not possible, I have for many years employed a method which has been giving excellent results, both theoretical and practical. By means of this method, which I have termed *hypnoidization*, I have been able to induce states closely allied to sleeping states on the one hand, and to hypnosis on the other. This work may be regarded as established, having since been confirmed by many other investigators, as well as by my assistants and collaborators in my laboratory.

Basing myself on the conditions which I have elsewhere described for the induction of states of normal and abnormal suggestibility, as well as for the bringing about of dissociated states in general, I have applied the same or similar conditions in cases where hypnosis was not possible. The result was the induction of peculiar states which I termed *hypnoidal*. The hypnoidal state is of an intermediary character, intermediary between the waking state on the one hand, sleep and hypnosis on the other. Here specially lies our present interest in these peculiar states. The patient or the subject while in this state hovers between hypnosis and sleep. Now he may be in a condition which can only be characterized as light hypnosis, and then again to one's surprise the person is found to be in his normal waking state. The state evidently is highly unstable.

The oscillations of the different states may be followed by variations in respiration. Thus the respiration is somewhat unequal in the waking state, becomes quietened and more uniform in hypnosis. When, again, in the hypnoidal state the respiration is slow and with little or almost no variations, it becomes disturbed when the patient passes into waking state, and becomes uniform when he passes into hypnosis or sleep.

The method of hypnoidization shows clearly how the conditions of normal and abnormal suggestibility are utilized and modified in the induction of hypnoidal or intermediary states. I say modified, because it is just on such slight modifications and variations of the conditions mentioned above that the whole matter of hypnoidal states hinges. A quotation from a previous work of mine giving a short description of the methods of hypnoidization and of the character of the hypnoidal state will be opportune here. "The patient is asked to close his eyes and keep as quiet as possible, without however making any special effort to put himself in such a state. He is then asked to attend to some stimulus, such as reading or singing, or to the monotonous beats of a metronome. When the reading is over, the patient with his eyes shut is asked to repeat it and tell what comes into his mind during the reading, or during the repetition, or immediately after. This should be carried out in a very quiet place, and the room, if possible, should be darkened so as not to disturb the patient and thus bring him out of the state in which he has been put. As modifications of the same method,— the patient or subject is asked to fixate his attention on some object, while at the same time listening to the beats of a metronome; the patient's eyes are then closed, he is to keep very quiet, while the metronome or some other monotonous stimulus is continued. After some time, when the patient's respirations and pulse are found somewhat lowered, he is asked to concentrate his attention on a subject closely relating to the symptoms of the malady or to the submerged subconscious state. In other words, the patient is in a hypnoidal state favorable for the emergence of subconscious experiences.

"The patient again may be asked to be very quiet, to

move, or change position as little as possible, and is required to look steadily into a glass of water on a white background with a light shining through the contents of the glass; a mechanism producing monotonous sounds is set going, and after a time, when the patient is observed to have become unusually quiet, he is asked to tell what he thinks in regard to his symptoms. In other cases it is sufficient to put the patient in a relaxed condition, have his eyes shut and tell him to think hard of the particular dissociated states."

Now in working with the method of hypnoidization I have often observed in using it that the patient at first tries to concentrate his attention and seems to fall into slight hypnosis, but pretty soon he is fully awake. In closely watching this condition I found that at first the patient attempted to fixate his attention, then lost control over it. His attention being relaxed he fell into a sleep-state, out of which he emerged again, owing to the partial presence of the idea of the necessity of concentration of the attention, as well as to the partial watchfulness present. It is this alternate and incomplete relaxation and concentration of the attention that keeps the patient on the borderland of wakefulness, hypnosis and sleep. In some cases the hypnoidal state passed into hypnosis. Thus in one of my cases, V. F., at first I obtained only hypnoidal states, but after some time the hypnotic state gained ground and the subject passed into typical hypnosis and finally into a somnambulistic state. In other cases I have observed that preliminary to the passing into the hypnotic state proper a short interval is present which may be regarded as a hypnoidal condition. In many other cases the patient is not in the hypnotic condition, but still there are phenomena present which remind one strongly of the hypnotic state.

The close relationship of the hypnoidal state and of hypnosis is sometimes forcibly brought to the mind of the experimenter. Some patients while in the hypnoidal state are observed to become unusually quiet, less talkative, relaxed and after a time distinctly cataleptic. The patient has apparently passed into hypnosis. In most of the cases the hypnosis is of very brief duration. On the other hand, in other cases the subject falls into a sleeping state without

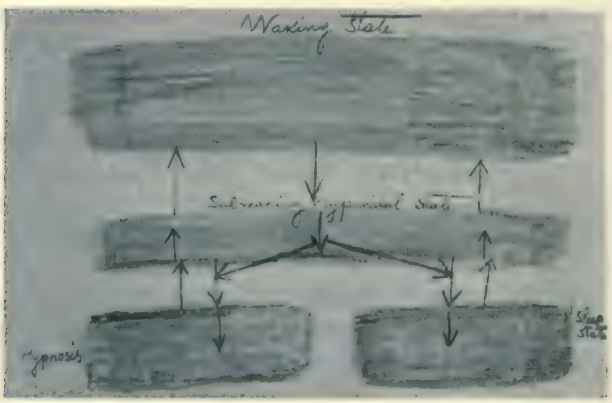
as much as touching on hypnosis. The hypnoidal state is on the borderland of waking, sleep and hypnosis. *Subwaking* seems to be an appropriate descriptive term of the hypnoidal state. Like sleep and hypnosis, the subwaking hypnoidal state greatly varies as to depth and duration: it may range from the full waking consciousness to deep hypnosis. The same patient may at various times reach different levels, so that not all the hypnoidal states are of the same depth; and in that respect they are very much like sleep and hypnosis, which really are not always of the same depth.

What is specially characteristic of the hypnoidal states is the difficulty of fixing them for any length of time,— they dissolve into mist as soon as an attempt is made to seize them,— they are extremely fleeting and evanescent. What specially interests us is the close relationship of the hypnoidal state with sleep-states. The hypnoidal state is the bridge that connects the waking state not only with hypnosis, but also with sleep. To enter sleep or hypnosis one has to pass through the intermediate state, the hypnoidal state.

We may also add that this holds true not only in the case of passing *into* any of the hypnotic or sleep-states, but also in the case of passing *out* of them. A close observation of cases will show that in awakening from hypnosis, as well as from sleep, there is present a short period occupied by a peculiar condition of consciousness, a condition which is no other than the hypnoidal state. It is such states between sleeping and waking, whether on the way to or from sleep, that may be designated as *subwaking*, and are akin to hypnoidal states.

The subwaking states are characterized by the trait of suggestibility. Suggestions are fully possible in such conditions. In fact, in all those cases where hypnosis is impossible or impracticable the hypnoidal states can be utilized for the same purpose. The subwaking or hypnoidal states are utilized by me and by many other investigators in a systematic and methodical way for the obtaining of submerged subconscious experiences. This clearly shows that the subwaking states, while being of the character of sleep-states, are also in close touch with states of dissociation.

One important characteristic of all subwaking states is the formation of hallucinations which indicate states of dissociation under which alone hallucinations can take place. On falling asleep hallucinations crowd consciousness, and on awakening a crowd of phantastic percepts, often giving rise to disconnected dreams, haunt "the halls of consciousness." We may possibly term the intermediary subwaking states leading into sleep as *hypnagogic states* and the dream-hallucinations formed *hypnagogic hallucinations;* the intermediary subwaking states which arise on coming out of sleep we may term *hypnapagogic states* (from ὕπνος, sleep; and ἀπάγω, lead away); the dream hallucinations formed may be termed *hypnapagogic hallucinations.* In passing from waking states into sleep and again from sleep into waking, we pass through those intermediary subwaking states. We may graphically represent those intermediary, transient, subwaking states as follows:




In other words, in going to sleep or rising out of it as well as in entering into hypnosis and its allied states and coming out of them the transitory, subwaking, hypnoidal states are passed through. The subwaking states may therefore be regarded as truly intermediate. The subject in passing through the subwaking hypnoidal states may either fall asleep or pass into hypnosis. Facts thus clearly indicate that sleep-states are closely interconnected with

intermediary subwaking states discovered in my investigation of the subconscious. In the study of sleep then we must devote our attention to the investigation of transitory subwaking states which form the transition between waking and sleeping.

## Chapter V

### *The Induction of Sleep-States*

OWING to the kindness of Dr. Walter B. Cannon, Professor of Physiology at the Harvard Medical School, I was given the opportunity and facilities to perform some experiments at the Physiological Laboratory of that institution. I wish to thank Dr. Cannon for all the kindness and consideration he has shown me in the carrying out of the experiments on sleep at his laboratory.[1]

Guided by the work, on account of which was given in the previous chapters, I undertook a series of experiments on the induction of sleep in different animals. I thought it might be well, since sleep belonged to animals as well as to man, to start my experiments on animal life, beginning with lower animals and ending with man. My experiments were carried out on frogs, guinea pigs, cats, dogs, infants and adults. It occurred to me to use the *same methods* to bring about sleep under the conditions of normal and abnormal suggestibility in all those various representatives of the ascending scale of animal life. I consequently tried to induce sleep under the conditions favorable for the bringing about of the intermediary, subwaking, hypnoidal states. Now I pointed out above, that the transient character of the subwaking hypnoidal states was due to the variability of the conditions under which they were induced. Hence the fact of oscillation or instability of such states, the subject now plunging into hypnosis, now into sleep, and then again emerging into the light of the waking state. Hence the mixed manifestations observed in the subjects, when in the hypnoidal state, now presenting the traits of hypnosis and

[1] I take here the opportunity to acknowledge my obligations to Dr. Morton Prince and to Dr. W. B. Cannon for the many helpful suggestions given to me in the revision of the manuscript.

now of sleep. My point therefore was to induce sleep and its allied states, subwaking and hypnoidàl, keeping as closely as possible to the conditions of normal and abnormal suggestibility.

Now I found in my experiments on the induction of the intermediary hypnoidal states in man that the conditions of monotony, limitation of the voluntary movements, limitation of the field of consciousness were of the utmost consequence. In my present experiments on animals I followed the same line of work and as far as possible reproduced the same conditions. I tried to limit the incoming sensory impressions, to limit the voluntary movements and thus produce a *monotonous* state by the continuous inhibition of new and varied stimulations. After narrowing down the animal's psychophysiological activity I invariably found that when I succeeded in maintaining closely the same conditions which had been found favorable in human subjects for the induction of subwaking states, the animal uniformly fell into a passive condition closely analogous to the subwaking state and in many instances into a deep sleep. The condition of the animal was often strikingly similar to the one observed in the human subject. The respirations and pulse were lowered, while mental activity in the higher animals, alertness of sensory and motor reactions to external stimulations in the lower animals became greatly reduced and even completely suppressed. In the higher animals, such as dogs, a transitory cataleptic state, a state in which the voluntary muscles retained the position given to them, could be observed accompanied by a disturbance of respiration and heart beat. The slight disturbance then subsided and calmness supervened. The calm lasted but for a brief period of time and the disturbances reappeared. The latter were once more succeeded by calm which ended by full waking state or by a deep sleep. In other words, the experiments, the details of which are adduced further on, allow me to draw the conclusion that I had here the typical manifestations observed under the same conditions in my human subjects. I found here the manifestations characteristic of the intermediary hypnoidal states, the animal now passing into waking and now falling into sleep.

Of course, in the case of the frogs the interpretation of the manifestations is rather uncertain. Still even in the frogs as the course of the experiments advanced the results stood out more clear and distinct, especially when viewed in the light of the experiments performed on the higher animals.

## Chapter VI

### *Experiments on Frogs*

IT is probably best to begin with the facts and give their interpretation afterwards. It seems advisable to give the reader a clear and full account of my work as a whole, to avoid being entangled in a mesh of unnecessary details. While I experimented on a great number of frogs I present here but a few typical experiments. The reader will thus be in a far better position to get a fair view of the work and be more enabled to judge critically the conclusions drawn from the experiments.

When the frog is put on its back, its lower lid (its only eyelid) is drawn up. If kept in the same position for a few minutes, the frog ceases to struggle and becomes quiet. The lower lid is kept shut, but when the lid is drawn down the frog becomes lively. If the frog is put on its back and stroked gently, the eyes close, the lower lid being pulled up. The frog becomes very quiet, but soon becomes again lively.

The frog was kept on its back for a few minutes; it was held down and its voluntary movements were restricted; it soon became cataleptic. There was a change in the respiration as shown by pneumographic tracings.

A cloth was wrapped round the frog's head. The frog remained very quiet apparently in a very deep "sleep." The eyelids were found drawn up. The same experiments were repeated on other frogs, small and large, and the same results were obtained.

When a cloth is put on the eyes of the frog so as to exclude all light, the frog sinks into a state of rest. It is cataleptic and can then be put in any unnatural position; it does not make any attempt to change its position. The frog reacts but feebly to sensory stimuli; the eyelids are found to be drawn up.

The experiments were then somewhat modified,—the eyelids of the frog were held together with collodion painted over them. At first the frog struggled and was very restless, but soon it became very quiet, apparently sunk into deep "sleep."

The more often the frog is put in that apparent "sleep" or rest-state the easier it is afterwards to induce the same condition.

A big lively frog was blinded. It was easily put in any position, though it would not have retained such awkward postures in its normal healthy state, before it was deprived of sight. It may be well to add that the experiments were performed after the frog recovered from the shock of the operation. The blinded frog could be manipulated far more easily than any other specimen of its kind. It could be placed in any awkward position and would remain so indefinitely until disturbed. No frog with its eyesight unimpaired would have remained for a moment in such awkward uncomfortable positions. The frog, for instance, was hung up by its front limbs on a board, it remained in the same position until disturbed. The frog was put over the edge of a jar where it hung with the front limbs, the body and hind legs relaxed in the jar. It remained in this uncomfortable position until disturbed.

The blinded frog was left in the jar for a few days and then the same experiments were repeated with the same results. The frog was turned on its back, it remained in the same position, as if frozen and turned to stone. The flame of a match was applied to him and he responded to the pain stimuli very sluggishly, but did not turn over. The respiratory and swallowing movements were reduced from 60 to 48 per minute. If left undisturbed, he would remain in the same position indefinitely.

The frog was put in a jar; he tried to jump out of it,—body and forelimbs were outside the jar, but the hind legs remained in the jar dangling. He remained in this position without any change. It seemed as if he fell "asleep" and "forgot" about the jumping out; it reminded one of the story of the sleeping kingdom.

To control the experiments on the blinded frog I took

a healthy frog which was not operated and closed its eyes firmly. It was put in a very awkward position. It remained hanging over the board without any movement for a period of 5 minutes. Respirations became very slow. In this position I could carry it round the laboratory without disturbing it in the least. After 5 minutes it opened its eyes slowly and changed its position.

The blinded frog was put in an upward sitting posture. Its back was supported by a board so that it should not tumble over. In this attitude it remained without change for a long time.

A healthy, not blinded frog, was then taken and after having closed its eyes firmly with my fingers for a period of a few minutes it was put in a sitting posture similar to the one given to the blinded frog. The healthy frog remained, as if stiffened in the same position. Respiration fell to 48 per minute. During the whole period of the experiment the respiration and heart beats were greatly reduced. Response to sensory stimuli was very sluggish; the limbs were in a state of relaxation.

After a few weeks the blind frog was still alive and kicking. It kept very quiet and could easily be put into a cataleptic state.

The blind frog remained in the same position, when left to itself. It remained in any posture given to it. When put over the edge of the jar, it remained in the same position without changing a single muscle. During this time it reacted very sluggishly to external stimulations. Respiration and heart beats were very much retarded.

Similar experiments were carried out by me on a number of frogs with the same results. In some cases interesting effects of inhibition could be observed. Thus in one of the actively respiring frogs with pronounced swallowing movements the latter ceased, when the muscles of the hind legs were seized and sharply and violently pressed or pinched.

Frogs were placed on their backs and then a heavy weight was put on them to hold them for a few minutes in the same position. The frogs soon fell into a cataleptic state and remained in that state even when the pressure was removed. The lower eyelids were found drawn up,

the eyes were partly closed. Some of the frogs when released from the pressure, relaxed their extremities and remained in a quiet state.

Similar experiments I carried out on a number of frogs; the results were of the same character. It is interesting to observe that when the frogs open the eyes and turn to their usual position they remain for some time very quiet.

There is one point to which I want to draw special attention and that is the significant fact that when the frogs are put in the cataleptic condition, and are from time to time restrained from righting themselves to their normal position, they finally fall into a very quiet state,— the limbs are relaxed and remain in a relaxed condition, the respiration and heart beat are greatly reduced and there is little response to external stimulations to which, though of slight intensity, they would have responded in their normal "waking" state by jumping away. In this respect I can fully confirm Huebel who pointed out this fact in his excellent work on the frog.

When I carried out the above experiments on the frog, I was unacquainted with Huebel's remarkable work. I was glad on reading that observer's experiments to find that the work done by me did not stand by itself, but that I had struck a path which had been trodden by a previous explorer having a similar goal in view. Now when I began my experiments on frogs I hesitated to speak of "sleep" in frogs. When however I continued my experiments, I could not help coming to the conclusion that there are such states as sleep in frogs and that those states can be induced under conditions very similar to those we had found in human beings. Still I greatly hesitated to term the states induced in frogs "sleep;" I termed them "rest-states." I had however a lurking suspicion that they might really be of the nature of sleep-states found in human subjects. I was therefore glad to find that Huebel had fearlessly and unhesitatingly described those rest-states by the term we describe similar states in human beings,— namely, sleep. I then took courage and walked with less hesitation and with more confidence on a path that had been unfortunately left untrodden and neglected by the foot of the scientific explorer.

I must, however, add that I am not quite so sure as Huebel is that the "rest-states," induced in the frog and described in my experiments, are sleep-states, but I do think that they are closely analogous to what we regard as sleep-states in the higher animals.

Without knowing of Huebel's work I came pretty nearly to the same results. I observed as Huebel had before me that frogs after they had been put in the characteristic cataleptic condition described by investigators, such as Czermak, Preyer, Danilewsky and others, that the frog passed into a quiet state, the limbs, though keeping apparently the same position, really not being any longer cataleptic, but rather relaxed, that the respiration and heart beats were greatly lowered and that if the frog could be left in this state without any disturbance, it would remain in that quiet condition for a very long time.

I did not observe as Huebel did that the frogs put in such states remained in it for over five hours, but I did observe that if the environment and external stimulations could be kept quiet, the frogs would remain in their passive states for a very long time. Unfortunately, the place where I worked was rather noisy; in fact unusually so, for the successful carrying out of such delicate experiments. I should not wonder then that I could not fully get all the results that Heubel got who was working under more favorable conditions. I am not quite ready to claim that the passive states in which the frogs fall are really sleep-states, a claim defended very strongly by Heubel, but I do favor Heubel's statement and think that the states of the frog, *after the cataleptic state has passed*, is very much like what is usually regarded in the higher animals as sleep.

Now if we scrutinize more closely the series of experiments carried out on the frogs, we find present the conditions of monotony and of limitation of voluntary movements as well as of limitation of what may be regarded in the frog as consciousness, or of the limitation of the activity of the sensorium by cutting off the regularly incoming sensory stimulations. As a result we find something analogous to what we should have expected in the human subject under like conditions, namely, the presence of peculiar passive

states,— that is all that we are thus far justified to say of them, observed and described as they are by experimenters who do not have the possibility of getting the subjective experience of the animal under observation.

What we find in the state of the frog is a condition somewhat analogous to what we have found in our experiments in human subjects, namely the *presence of intermediary states of the subwaking or hypnoidal type.* The symptoms observed differ somewhat, but in general they may be regared as alike. We find a passive state with cataleptic manifestations. The state varies from catalepsy to relaxation or what may be regarded as lethargy, and again from passivity to activity, from sluggish to very lively reactions in response to external stimulations. We have therefore here, manifestations which remind one of like manifestations in the human subject, namely subwaking, hypnoidal states which are on the borderland of waking, hypnosis and sleep. Of course, we should not expect to find that frogs which stand so low in the scale of vertebrates would manifest phenomena of the same character as the higher vertebrates, but we should expect that some similar phenomena, though otherwise widely different, would be present. This is precisely what we find in the frog. We find the *general* characteristics, though rather vague, of what is afterwards fully developed in man as the subwaking, hypnoidal state.

We must remember besides that the hypnoidal state is very unstable and its manifestations, having the characteristics of waking-state, sleep and hypnosis, greatly vary in different individuals and at different times in the same individual. We should therefore expect that the hypnoidal state would show still more radical differences from the typical in the various species of animals, especially in those that stand so far apart from each other as frog and man. What is surprising to me is not the fact of the variation and great difference of the hypnoidal state in the frog as contrasted with man, but that the difference is really not far greater, considering the gap that exists between the two organisms. In fact, the similarity is far more striking than is the difference between the hypnoidal states of the two

contrasted organizations so widely apart in the scale of evolution.

My view then is that *the phenomena observed in the frog are hypnoidal in character.* The phenomena themselves as well as the conditions under which they are induced warrant my view of the hypnoidal nature of the states.

In this respect we can well understand the apparent disagreement of the early observers on the subject. Czermak and Danilewsky regard the phenomena as being of the hypnotic order, Heubel regards them as being more of sleep-states, while Preyer views them as being the results of fright which give in the waking-state cataleptic manifestations closely similar to those observed in hypnosis. Verworn who regards the phenomena as "Lagecorrectionen" due to central inhibitions really does not conflict with any of the views. It is simply a general physiological hypothesis which may be in accord with any view, a physiological hypothesis which may or may not be true; it is a hypothesis far removed from the special facts and should be tested on its own merits. My point of view is not a matter of hypothesis, but describes and explains the phenomena in terms of states having similar manifestations and produced under the same conditions, states which are more developed and stand out more pronounced in higher animals. These states possess many of the characteristics of the waking state, sleep and hypnosis. Hence the reason why the early observers regarded the phenomena as waking states, as Preyer did; others regarded them as hypnosis, a view maintained by Czermak, Heidenhein, Danilewsky; while other investigators regarded the phenomena as sleep-states. As a matter of fact the phenomena and the conditions under which they are induced make the view highly probable that the different investigators are not far away from the truth, but not being acquainted with the peculiar hypnoidal states described, they observed the phenomena in too one sided terms, in terms of sleep, or hypnosis, or of waking-states. In reality the phenomena and the conditions under which they are induced point strongly to the fact that the states are hypnoidal in character, states which partake at once *of all the three apparently contradictory manifestations,— waking,*

*sleep and hypnosis.* Now the manifestations of the waking-state, and now the symptoms of sleep, and now again of hypnosis predominate. In short, the state induced in the frog under the conditions of monotony, limitation and inhibition is a *variety of the subwaking, hypnoidal states.* This induced hypnoidal state being intermediary in character may either partake of the catalepsy of hypnosis strongly modified and manifesting itself differently in the frog than in the human subject, or may again go over into the passive state of "sleep" or some state analogous to it.

It is perhaps of importance to call attention to the significant fact that the first stages induced in the frog are rather of an unstable character,— the frog when put on its back and kept down for but a short time falls into an apparaently cataleptic state of short duration. The animal soon rights itself and is fully awake as before. This instability is very characteristic. Now the hypnoidal states are just characterized by this fundamental trait of instability. It is only when the condition of monotony, limitations of voluntary movements and inhibition are sufficiently prolonged that the catalepsy becomes more or less fixed for some period of time, and when this passes off, and the conditions under which the frog is kept are continued still further, it is only then that the frog sinks into a passive state which may last indefinitely, unless brought out of it by some strong stimulation. It seems to me then, that if we take all this into consideration, we cannot possibly describe the state in which the frog is put in other terms than what we have on other occasions discovered to be the intermediary, subwaking, hypnoidal state.

## Chapter VII

### *Experiments on Guinea-Pigs*

WITHOUT going into physiological speculations let us once more return to our facts.

In the pathological Institute of the New York State hospitals, I had occasion to carry on a few experiments on guinea-pigs. One of the guinea-pigs was put into a cataleptic condition by gentle stroking, while another was put into a similar state by simply seizing him suddenly. The guinea-pigs lost the control of their extremities and could not move even when stimulated by strong electric currents. After a few minutes left in the same position the guinea-pigs recovered the use of their extremities. The holding down of the animal to one position of the limbs, the gentle stroking, or the strong emotion of fear favored the condition of monotony and inhibition. In my present experiments I tried something similar, namely, to put the guinea-pig under the same conditions of experimentation under which I put the frogs and my human subjects.

A guinea-pig was bound with straps and plaster strips and was left in this condition over night. In the morning he was found in the same place,— he could not free himself. He was very much weakened and paralyzed. He did not struggle much and could easily be put into a passive state. Unfortunately it was hard to say whether the passive state was due to his weakness or to the conditions under which he was artificially put.

Young guinea-pig, of a few days old; very lively. I took him in my hands and kept him quietly and then put a blind on his eyes as I did in the case of the frogs. At first he struggled, then became very quiet. Respiration fell from 120 to 90 per minute. He remained in the same posture for about 35 minutes. He did not respond to stimuli such as intense noises. After fifteen minutes respiration was regular and fell to 60 per minute. The guinea-pig looked like a ball of immovable matter,— the

only thing indicating life was the regularity of the respirations. After a few minutes I found that he started at intense stimuli, but still did not change posture. As he was near the end of the board his foot slipped from relaxation of the limbs, he changed posture, but remained immovable. After half an hour he began to stir and move his head slightly.

On closing the pig's eyes there was a difference in respiration,— the respiration fell and became more uniform.

I tried now to modify somewhat the experiments. I found that the pigs were extraordinarily lively animals and struggled a good deal. Now in my experiments on human beings where there was too much opposition to the induction of subwaking states, I could reduce the opposition by the use of hypnotics and anaesthetics. I tried similar experiments on my frogs, but the results were not satisfactory. The anaesthetized frogs either reacted very much like normal animals, or when given a somewhat larger and more effective dose soon died. I attempted to paint the skin of the frog with collodion to exclude stimulations of the skin, but the frogs died in convulsions. When I soaked their skin in chloroform or ether I had the same results. Evidently the skin of the frog absorbed the chloroform or ether and produced undesirable poisonous effects. Since however I did have success with my human subjects under similar conditions, I thought I might have some favorable result in the case of the guinea-pigs.

A guinea-pig was put under chloroform for a few seconds. Respiration fell to 48 per minute. Reflex movements were well preserved. Eyes were partly closed.[1] The guinea-pig started every time, but did not react to sound or light-stimuli, and very sluggishly reacted to pain-stimuli. He shivered and then closed his eyes. About 15 minutes later he began to react to stimuli. The reactions conformed to Pflüger's law[1]. Stimulations called forth reactions on the same side. About half an hour later the pig began to feel pain, cried on sharp pinching of the front paw. Respiration was still greatly reduced. By closing his eyes and by continuous gentle stroking of his back, it was easy to make

[1] See p. 87, footnote.

him fall asleep. He rolled up in a ball and seemed to sleep quite comfortably.

Guinea-pig under ether for about a minute. Respiration and heart-beat greatly lowered. Muscles were relaxed; eyes were half closed in sleep. Later on he awoke and was fully alive to external stimulations, though not so lively as usual. I put him in my hand, kept him very quiet and closed his eyes. He evidently fell asleep as he did not open his eyes when I removed my hand from them; on the whole the pig kept unusually quiet without stirring a muscle.

A guinea-pig was put under ether for 30 seconds. Reflex of wiping with forepaws was present. Reactions to stimuli were rather sluggish. After a few minutes the pig awoke, but had a tendency to go to sleep again when I put him in my hand, kept him very quiet and closed his eyes. He fell into a quiet sleep, his muscles relaxed; respiration and heart-beat were much lowered. When disturbed, he became fully awake and began to eat, but went immediately to sleep again, when put in my hand and kept quiet. Now and then a passing state of resistance of limbs was observed. The limbs once changed often retained the posture given them. This state however was far from being as marked as it was found in the case of the frog.

When light was excluded from one of the eyes of the animal the other eye began to contract and close; the animal became unusually quiet. Anything that excluded visual sensory stimuli and brought about limitation of the voluntary activity of the pig produced a state very much akin to sleep with now and then a slight catalepsy quickly followed by relaxation of the limbs.

A guinea-pig was fixed on a board,— he kicked violently. Collodion was used to fasten his eyelids together and as in the case of the frog, he became quiet and ceased to make attempts at fighting.

A guinea-pig breathed chloroform for two seconds only. It seemed to have affected the animal sufficiently to make him quiet. He did not fight; lost a good deal of his liveliness. I put my hand on him, restricting his movements. He did not resist; I then closed his eyes; he remained without stirring in the same posture.

When I closed one eye of the animal, the other closed also and the animal seemed to have gone to sleep, the limbs being in a relaxed condition.

After some time he opened his eyes again; chewing movements were present; active, lively and restless, sniffing about in all directions. I once more took him in my hand, kept him very quietly, but firmly,— his activity subsided, he ceased to struggle, eyes became contracted as well as pupils; finally eyes closed very slowly. The animal sank into a state of torpitude. Respiration and heart-beat fell.

Thus far we may say that the experiments on guinea-pigs gave results somewhat similar to those of the frogs, though the cataleptic states were not so pronounced,— in fact they were very transient. Still the induction of sleep was brought about under conditions of monotony, limitation and inhibition. It was far more difficult to bring about rest or passive states in guinea-pigs than in frogs, on account of the great liveliness and ceaseless activity of the pigs. It may be objected that the anaesthetics somewhat modified the result, because it might be claimed that the sleep-states induced were really due to the anaesthetics used. This objection however can be easily obviated by the rejoinder that the action of the anaesthetic was only to reduce the extraordinary activity and restlessness of the animal and thus make it easier to induce sleep. The sleep-states themselves were really produced under the same conditions as were the ones induced in frogs and in my subjects. In fact even when the guinea-pigs were really lively and active it was sufficient to subject them to the conditions described, when they gradually fell into a state very much of the character of hypnoidal states and sleep. The phenomena though were not so well marked as in the frogs.

# EXPERIMENTS TO DETERMINE CO-CONSCIOUS (SUBCONSCIOUS) IDEATION

BY MORTON PRINCE, M.D.

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ALTHOUGH a large and still accumulating mass of evidence has shown, under certain pathological and other conditions, the presence of co-conscious (subconscious) processes of which the subject is unaware and yet which manifest themselves through intelligent actions, still all writers are not in accord as to the interpretation which shall be put upon these manifestations. While I believe all students of abnormal psychology — those who have done the experimental work — are in agreement in interpreting subconscious manifestations as the expression of subconscious ideas more or less dissociated from the personal consciousness — that is to say, they are agreed on the psychological interpretation — there are certain theoretical psychologists who still insist that all such subconscious manifestations are compatible with the interpretation that they are a result of physiological processes without any association with ideas whatsoever.[1] This is the physiological interpretation. It is, therefore, desirable to obtain evidence which will determine which of these two interpretations is correct.

The following experiments were undertaken to obtain, if possible, such evidence.

B.A. is a case of multiple personality of which one personality may be designated as A and the other as B. Observation extending over many months has shown that A has no knowledge of B, but B is completely aware of A; that is, A has amnesia for the state of B, while B has no amnesia. B can be hypnotized and in hypnosis is known as "*b*." On awakening from hypnosis B has no recollection of *b*. Now the important point for our present purpose is

[1] See Symposium on the Subconscious. JOURNAL OF ABNORMAL PSYCHOLOGY, Vol. II Nos. 1 and 2, 1907.

that B, both when awake and when in the hypnotic state as *b*, claims to be subconscious (co-conscious) with A when that state is to the fore; she claims to have perceptions, feelings, and trains of thought distinct from and synchronous with the mental life of A; she describes them with precision and specifically.

Of all this claimed subconscious life A has no knowledge. The question is, can B's (and *b's*) claim be verified even in part? She herself says: "I *know* it is so, but that is not proof for another person."

To obtain evidence that would justify the inference, I devised the following tests: —

### TESTS IN CO-CONSCIOUS PERCEPTION.

*Experiment* 1. The following experiment was made without the knowledge of any of the personalities.

While talking with A, I had her close her eyes for a moment and while they were closed, as silently as possible, without moving from my seat, I hung a handkerchief over the back of a chair on my left within the range of her peripheral vision. I then directed her to open her eyes, to keep them fixed on mine and to listen to what I had to say. I talked to her earnestly for a few moments, holding her attention. She kept her eyes fixed on mine and did not once look towards the handkerchief or allow her eyes to deviate. (As every physician knows, it is easy to detect the slightest deviation of the eyes under such conditions.) After the lapse of about half a minute she closed her eyes at my request and I surreptitiously removed the handkerchief. Then, after allowing her to open her eyes again, she was directed to describe any change she had noticed in the environment while I was speaking to her; anything she had seen which had not been present before closing the eyes the first time, or anything which she missed now. She was unable to describe any change; she had seen nothing. She was now hypnotized; the hypnotic state *b* immediately said on coming: "I know; there was a handkerchief hanging on the arm of the chair; I saw it. A did not see it, or at any rate, did not think about it, for I only know what she sees by what she thinks." As to the content of *b's* co-con-

scious perception she testified, "There was no thought in my mind connected with the handkerchief, excepting I thought Dr. Prince's handkerchief was hanging on that chair. I didn't know it was any part of an experiment,—I thought you were going to hypnotize her without her knowing it. It seems to me that I was there as a personality, that I am there all the time, just as I am here now, only now I can move and do as I please. When A is here I can't." Here it is interesting to note that two other hypnotic states which can be obtained in this case did not see the handkerchief, but neither of these claims to be or has shown any evidence of being co-conscious with A. Their testimony, however, corroborates A's statement of not having seen the handkerchief. Both these hypnotic states, known as *a* and *c*, have a complete knowledge of A's consciousness, or rather, *are* A hypnotized. Neither saw the handkerchief.

*Exp.* 2. A similar experiment was made under the same conditions with (the real) Miss Beauchamp. Awake as Miss B. she did not see the test object, but the hypnotic state B II described it. Afterwards Miss B. could not be made to recall having seen it, though it was described to her.

Accidental failure on the part of Miss Beauchamp to notice objects, such as passing me in the street, with complete perception by some one of her subconscious states was frequently observed; but, of course, it is not possible to exclude amnesia as a possible explanation. Amnesia is even a possible interpretation in the above two experiments. It was, therefore, necessary to test the subconscious by methods in which amnesia could not be a factor.

### CO-CONSCIOUS PERCEPTION AND REASONING

The following experiments were made without A's knowledge of what was being done. C, and afterwards B, were informed of the nature of the tests and entered into them with interest. The point of the experiment was to communicate to subconscious *b*, without A's knowledge, a mathematical or other problem, of which even b did not previously know the factors. This had to be done while A was thoroughly alert, and without in any way disturbing (dissociating) her consciousness or allowing her conscious-

ness to become aware of and thereby possibly take part in the problem. *b* must, thereforc, subconsciously become informed of the given problem without A being informed at the same time. The problem also had to be one which presumably could only be performed by psychological processes so far as we know.

Accordingly it was agreed with *b* that, when co-conscious, she should do a particular sum in artihmetic while A's consciousness was engaged in another task. The sum would be to add mentally certain numbers and, as a more difficult problem, to calculate the number of minutes or seconds intervening between two given hours. The figures to be added and those indicating the times were not to be given until A was present so that *b* thus far knew only the nature of the task, not the task itself. It was necessary, of course, to convey to subconscious *b* the information, without at the same time informing A, and without producing any of those artificial dissociations which frequently follow when communicating with the subconscious.

It was accordingly agreed that A's attention should be engaged by having her write some verse with which she was familiar on a sheet of paper, on the upper and lower margins of which the required figures for the sum would be unobtrusively written. Co-conscious *b* was to perceive these figures and make the computation. It was expected that A would not notice the figures if her attention by a little art was centered upon the verse as the important thing, but at the same time it is obvious that it did not matter if she did, as the figures could have conveyed no idea to her, either as to their meaning, or as to the nature of the task, or as to whether she was expected to do anything with them, for A was kept entirely in the dark regarding the nature of the experiment, and was not even informed that an experiment as such was being made. If she had seen the figures they could have meant nothing to her

On the other hand if *b* was truly subconscious, as she asserted, she should be able to recognize the figures, interprer them, and possibly make the computation. It was furthet arranged with *b* that as soon as A had completed the verse she was to be changed to *b* who was to give the answet

immediately on "coming," without delay, before she could have time to make the computation from memory after the change to *b*, in case it had not been done co-consciously.

*Exp.* 3. A was brought and the number 53 was written, without her knowledge, in the left-hand upper corner of a sheet of paper measuring $8\frac{1}{2} \times 11$ inches, and 61 in the lower right-hand corner. The task for the co-conscious was to recognize these numbers and to add them together mentally.

A was then directed to write in the middle of the sheet the verse: "Tell me not in mournful numbers," etc. I distracted her attention, as a conjurer might, so that she would not and, apparently, as a fact did not examine the paper, though as already said, it would have been immaterial if she had.

As she wrote she repeated aloud in all the experiments, whatever verse was chosen, sometimes commenting on the poet's thoughts, sometimes on the faults in her own memory as to the words, somtimes laughing at both. This is of importance as indicating that she was *alert* and did not go into a dreamy state as some automatic writers do.

Once only (the first experiment) she stumbled a bit while writing, as if there was a momentary inhibition of her thoughts by the subconscious process. A different verse was selected for each experiment.

After the verse was finished, when questioned she stated that she had not noticed, in any of the experiments, any other writing (the numbers) on the paper which had appeared quite blank.[1] This was the case in every instance. Whether this was due to my distracting her attention or to the effect of what might have been equivalent to a post-hypnotic suggestion, due to the conditions of the experiment, is not material. She was then changed to *b*. Immediately on appearing *b* exclaimed, almost shouted: "114," which is correct.

In addition to this sum in mental arithmetic it was arranged that *b* was, subconsciously, to count the buttons

[1] This was probably due to the fact that I had picked up intentionally, but with apparant carelessness, a wad of loose sheets of paper lying on my desk, some of which were blank and some written upon (notes of experiments, etc.), so that there was nothing peculiar in a couple of numbers written on a sheet to strike the attention or arouse suspicion.

on my vest (previously concealed) and multiply the number by six. *b* gave the number of buttons correctly in the same breath with the above sum — but, as she explained, she forgot to multiply by six. As to the way she subconsciously did the sum, she explained that she visualized the numbers, though they did not arrange themselves in a definite form before her mind, and the sum then did not do itself, as was the case with another subject *b*; but she said to herself: "Three and one are four, six and five are eleven," etc. "I looked up and I looked down at the numbers, and I looked at the buttons on your vest." (I had noticed a movement downwards of the eyes towards the lower number, but did not detect the other movements; but, of course, the eyes were in constant motion as she wrote.)

*Exp.* 4. The numbers 1.20 and 2.47 were written at at the top of the sheet of foolscap (8 × 12 in.), thus: 1.20
2.47
These indicated twenty minutes past one, and forty-seven minutes past two, and the problem for the subconscious was to calculate the difference in time in minutes. On appearing *b* promptly gave the answer: "one hour and twenty-seven minutes."

*Exp.* 5. In the same way the numbers 3.15 and 4.33 were written. The problem was, as before, to calculate subconsciously the number of seconds interevning between these hours. *b* correctly gave the answer: 4680. She further described her subconscious mental processes in doing these calculations. She said, for instance, to herself: three-fifteen to four-fifteen is one hour or 60 minutes; four-fifteen to four-thirty-three is 18 minutes. 60 and 18 make 78. 60 times 78 is 4680. (This multiplication was done in the usual way, *i.e.*, $6 \times 8 = 48$; $6 \times 7 = 42$; $42 + 4 = 46$, etc.)

Personality B was now brought and her memory for the subconscious processes tested. (It will be remembered that B claimed to remember the subconscious states of A as a part of her own personality.) B thus far knew nothing of the experiments. All the details had been arranged with her hypnotic state *b* of whom, of course, she had no memory.

B now remembered the numbers which had been

written on the otherwise blank sheets on which A had written the verse, and that she had seen them, *but she remembered nothing of the subconscious calculations*, did not even know that there had been any calculations or that the numbers meant anything. In fact, she thought the figures stood for dollars and cents, as suggested by the decimal point.

The evident reason for this was that the calculations formed part of the system of ideas forming the synthetic group *b*. With the awakening of synthesis B, this calculating group naturally became dissociated as is always the case on awakening B out of hypnosis (*b*).

To prove this principle the following experiment was made. This time the directions being given to B instead of *b*.

*Exp.* 6. The following figures were written as before:

12.11

1.20

The problem was to calculate the number of seconds intervening between these hours. After A had written her verse, she was changed directly to B who immediately gave the answer 4860.

B now remembered not only the perception of the numbers, but the calculation, which she said had been difficult. She had made the number of minutes 81 instead of 79. This was due to the fact that she had taken the hour 1.20 as 1.30, then calculating, 12.11 to 1.11 is 60 minutes. If it had been 1.10 there would have been 20 minutes more (to make 1.30), but it was 1.11; so she should have subtracted 1 minute — instead of which she added 1 minute, making 21 minutes in all (instead of 19). $60 + 21 = 81$; $81 \times 60 = 4860$. This answer was not correct, but the error was even more instructive than if it had been right. The answer should have been 4940.

In order to meet the possibility (however improbable) of *b* and B having made the calculation after "coming," notwithstanding the shortness of time at their disposal — a few seconds — or during the transition period when A's consciousness was partially (or wholly) extinguished[1] it was arranged that the computation should be written auto-

[1] During such a transition period, however short, it is obvious that the secondary personality ceases to be co-conscious and becomes an alternating personality.

matically while A was conversing. The figures were 1.43 and 3.39. As a result the hand wrote out the calculation while A was alert as before but, of course, without awareness of her hand.

There was a slight error in the answer, the hand making the elapsed minutes 114 instead of 116. This obviously is is of unimportance. Of importance is the fact that the *hand* explained, in part, the steps of the problem, thus: "1.43 to 3.39 would be two hours less than [four minutes]." (This sentence was unfinished.) Then the number 114 was written and multiplied by 60 which was placed under it in the conventional manner and the answer 6840 obtained thus:

114
60

---

6840[2]

It must be remembered that the subject did not look at the paper while the hand wrote, so that the "co-conscious ness" was obliged to keep the figures in her head, though she calculated on paper. B and *b* later explained that when doing a calculation co-consciously on paper she could not visualize the figures as she did when she did the calculation mentally. She, therefore, could not keep the figures in her head. It was "like throwing them away," *i.e.*, they would drop out of her mind.

A number of similar experiments in which the calculations were written automatically were made. The results were substantially the same, the multiplication being always correct, though the elapsed minutes were wrong.

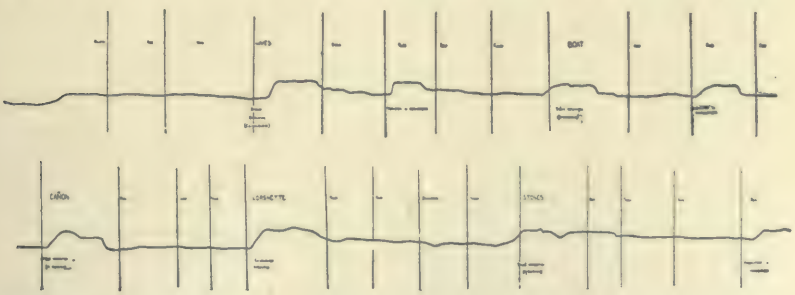
Again; I taught B some dozen characters of a shorthand which I make use of in note-taking. Most of these symbols I believe are purely arbitrary, of my own invention, and B A had never seen them before. As soon as B had memorized them I changed her to A and wrote the following: "I ⌊ | a ✓ of | " This was shown to A, to whom it meant nothing; but the hand promptly translated it, writing automatically: "*I have had a time of it.*" (Correct.)

[2] On the first trial the hand made the mistake of multiplying twice by 60. (The second time incorrectly.) When told "wrong," it wrote: "I can't do it as well when it is on paper. Are the hours right? 1.43 to 3.39." Then, as above, multiplied 144 by 60 correctly.

Now here something outside of A's consciousness recognized the characters, read them and translated them; something that in our experience can only be defined as intelligence.

Considering the conditions under which these experiments were made; that A knew nothing of their nature, that *b* and B did not know beforehand what the particular problem was to be; that the figures were not visible until A began to write the verse, and that A did not know the short-hand characters, — the conclusion seems inevitable that they, figures and symbols, must have been interpreted subconsciously even if A saw the former (which apparently she did not do); for A could not have known what any of them meant, and that the calculations and translation must have been made co-consciously; accordingly the memory of *b* and B, when afterwards describing the calculations, must have represented the facts. That such perceptions, interpretations, calculations and translations could have been made by pure *physiological processes without thought* is inconceivable and not substantiated by anything that we know of physiological processes.

In conclusion, I may cite one of a series of experiments made in conjunction with Dr. Frederic Peterson by means of the Psycho-Galvanic Reaction Method to determine the presence of subconscious emotions of which the subject was unaware. This method, it will be remembered, depends upon the fact that an electric current, which is made to pass



through the body, is increased whenever an emotion is aroused. The emotions are excited, as in ordinary association experiments, by test words which are given to the

subject interspersed among indifferent words. The increase of current is recorded on a kymograph and appears as waves in the tracing. Advantage was taken of the fact that the hypnotic states, *a* and *b*, remembered dreams of which the subject was unaware on awaking. In one or the other of two night-mares, a boat, waves, a cañon and stones played a strongly emotional part and these words, with the word "lorgnette," were interspersed in a list of twenty-five words. The word "lorgnette" referred to a subconscious experience of *b* made up of perceptions and thoughts of which the waking personality had no knowledge.

The tracing obtained shows very marked rises in the curve corresponding to the words "waves," "boat," "cañon," "longrette," "stones."

The subject herself had, connected with these words, no associations which could explain the waves in the tracing and was not consciously aware of an emotion.

ERRATUM:— By a clerical error in transcribing the tracing on page 41, the vertical lines indicating the chronological position of the test words have been displaced, for the most part to the right, so that the words, "boat" and "stones," for example, are made to appear during the rises of the curve instead of shortly before, as was the fact. The correct relations will be shown in the account of the experiments to be published later in full.

# THE GALVANOMETER IN PSYCHOLOGY

BY FREDERICK PETERSON, M.D., NEW YORK

[Demonstration of the apparatus and its uses before the New York Neurological Society, November 12, 1907.]

IN the transactions of the *Société de Biologie* for 1888, page 217, Féré made a brief statement that a deviation of the needle of a galvanometer was produced under the influence of the emotions. In 1890 Tarchanoff, a Russian, published in *Pflüger's Archiv fur Physiologie* an article wholly devoted to this influence of psychic processes upon the galvanometer. His discoveries lay buried for years until Sticker in 1897 corroborated them (*Wien. Klin. Rundschau*, Nos. 30 and 31, 1897). His work was also neglected until 1902 when Sommer of Giessen (*Beit. zur Psych. Klinik*, 1902, 2. 157) investigated the subject and upon the whole condemned it. In 1904 an electrical engineer, E. K. Müller, of Zurich, rediscovered these phenomena while experimenting upon himself in relation to the resistance of the human body in the alternating magnetic field. He told his novel experience to Dr. Veraguth, a neurologist of Zurich, who thereupon began experiments which fully established some of the facts as previously set forth by Tarchanoff and added others. His first article on the matter was published in the *Arch. de Psychologie* of Geneva, in August, 1906. In the winter of 1906 and 1907 Dr. Jung of the Psychiatric Klinik at Zurich and myself undertook a series of experiments in this connection which were published in *Brain* for July, 1907. The results obtained are briefly as follows:—

The mirror galvanometer of Deprez-d'Arsonval made by Carpentier of Paris (model A, suspension thread 0.08 mm., duration of simple oscillation 1.69 seconds, resistance 6.2 ohms) is used, and the reflected light from the mirror is either thrown upon a screen by means of an arc light or upon a millimetre scale. For scientific purposes the latter is the better method, for by means of a sliding index connected with a pen, permanent records may be made upon

the kymograph. We are hoping to secure an automatic recorder, such as is used in cable offices, which will greatly simplify registration.

The test person is put in the circuit with the galvanometer and one dry cell and a shunt is also employed to regulate the amount of current. The electrodes are nickel-plated copper upon which the hands or feet may rest. I have used one electrode of carbon and another of zinc in which case a cell in the circuit is unnecessary. The fluctuations observed are due to varying resistance in the human skin. Moving the fingers or lifting the hand causes the light to recede because of increased resistance. Very strong pressure may diminish the resistance and cause the light to advance. Such muscular movements are readily detected after some experience with this instrument and do not complicate or interfere with the movements due to the emotions. I have tried water-contact which excludes changes due to varying pressure. If the hands are blanched with an Esmarch bandage the deflections are not affected. If they are completely dried by immersion in formalin, the resistance is so great that fluctuations do not occur. A deep inspiration or a deep expiration or coughing will cause a deflection, whereas ordinary respiratory movements do not affect the movement. All of these influences are readily distinguishable by practice.

If a test-person is placed in the circuit and every possible stimulus avoided we obtain a rest-curve, which is characterized by first a considerable deflection and then a slow recession of the curve to almost nothing, which is quite contrary to our ordinary experience with cutaneous resistance. We should usually expect with a constant electromotive force and gradual reduction of resistance a gradually increasing deflection. When any stimulus is applied, sensory or psychic, we have after a latent period of 2 to 5 seconds a rise in the curve and then a gradual fall to the previous level. The stimuli which we made use of for illustration were sensory and psychic. We noted that expectation or anticipation, that embarrassment and apprehension caused fluctuations. A sudden sensory stimulus aroused attention, so that all of these stimuli have a psychic element. We may

place a patient in the circuit and ask him questions calculated to arouse an emotion and watch the deviations of the mirror, or we may propound to him words by the word-association method, words which when they touch upon emotional complexes create a movement in the galvanometer, corresponding to and corroborating the retarded reaction-time which also indicates the emotional value of the test words.

Now as to the actual practical value of this instrument in psychology, we have at least in it a new instrument of precision, which seems to be more accurate and more certain than any other employed in psycho-physical investigation, such as the pneumograph, the plethysmograph, ergograph, etc.

It promises to have a field of usefulness in all conditions in which study of the emotions or emotional complexes is of importance, as for instance in neurasthenia, hysteria, certain mental disorders, suspected criminality and malingering. I say it *promises* to have such a field of usefulness, for it is as yet too new and untried to permit of claiming so much as established. The psychometer may ultimately do more than we expect, or it may do less than we at present surmise.

The explanation of its action is not yet clear. So far as we know now, the deviations of the galvanometer are due to variations in resistance of the skin through an intimate connection of the sweat-secretory system with the nervous system, perhaps through the medium of the sympathetic nervous system. Yet it is also quite possible that some extra electro-motive force either from the tissues or from the cutaneous surface is added to the electro-motive force of the single cell employed in demonstration.

# REVIEWS

THE DANCING MOUSE: — A STUDY IN ANIMAL BEHAVIOR. *By Robert M. Yerkes, Ph.D.*

In this interesting monograph the author not only presents the first extended account in English of the curious behavior of the Dancing Mouse, but makes a valuable contribution to the methods of comparative psychology in general. The Dancing Mouse comes originally from China or Japan, and is characterized by extreme restlessness and excitability which it manifests in the peculiar whirling or circus movements rather inaptly called dancing. This activity is particularly noticeable in the afternoon and evening. The direction of the whirling is not constant, for some individuals ordinarily whirl to the right, others to the left, and still others whirl in either direction.

Various European investigators have surmised that the whirling was due to some congenital defect of the semi-circular canals, and have made extended anatomical studies of these organs. Their accounts are conflicting, and after careful survey of the results the author concludes that we must look for the origin of the behavior not to any abnormality in the structure of the canals, but rather to a general muscular and nervous instability of the organism. Some defect of the ear there must be, however, for all observers agree that the adult dancer is totally deaf. In the case of the young animals, on the other hand, the author found evidence of hearing. Of sixty-two young mice tested, forty gave unquestionable auditory reactions between the thirteenth and nineteenth days of age. After this no sign of hearing could be detected.

Of interest both for the understanding of the animal's behavior and for comparison with other animals and with man is the study of their brightness discrimination. Confined in a box with two exits, one right, the other wrong, can the mouse learn to choose the proper exit solely on the basis of difference in brightness? As an additional incentive to careful discrimination, the animal was given a disagreeable electric shock in case it made a wrong choice. This method of punishment for wrong choices was so successful that extensive use was made of it throughout the experimentation, and its application promises to be fruitful in further investigations. The results show that

the dancer can distinguish white from black very accurately, that after ten to twenty trials the animals will choose the right exit more often than the wrong one, and that after eighty to one hundred trials the right one will be chosen every time. Less training was necessary to establish a correct choice when the proper exit was black than when it was white.

The next question to present itself was what is the delicacy of the dancer's brightness discrimination? This the author tested with graduated gray papers and with an electric light box whereby the intensity of the illumination could be varied at will. Under the latter condition the human eye could detect a difference of one-twentieth, but the best that the dancer could discriminate, even after long training was a difference of one-tenth. Delicacy of discrimination in the dancer, therefore, is only about half that found in man. It is interesting to note that training produced a decided improvement. Speaking of his best subject, the author says, "At the beginning of the experiments a difference of one-half did not enable her to choose as certainly as did a difference of one-tenth after she had chosen several hundred times." In spite of considerable variation the author believes that the dancer follows Weber's law in brightness discrimination.

From many hundreds of experiments on color vision the author concludes that "the dancer is not able to tell green from blue, or violet from red," yet red could be discriminated from either blue or green. It is doubtful, however, whether this is true color discrimination, as red could not be distinguished from black. "Although the dancer does not possess a color sense like ours, it probably discriminates the colors of the red end of the spectrum from those of other regions by difference in the stimulating value of light of different wave lengths, that such specific stimulating value is radically different in nature from the value of different wave lengths for the human eye, and that the red of the spectrum has a very low stimulating value for the dancer." Examination of the structure of the retina by Dr. Waugh showed only a single type of visual element, a rod-like cell, and nothing corresponding to the cones of the human retina. This affords interesting confirmation of the Ladd-Franklin theory of color vision.

That the dancer learns, and learns very readily, is shown by the discrimination experiments. To what extent does it profit by imitating the acts of other mice? This was tested in many

ways by the author,— in discrimination experiments, in labyrinth learning, in ladder-climbing, in opening doors, etc., with the result that in no case was there any evidence of learning by imitation. The learning process is accelerated, however, if the animal is put through the act by the experimenter.

Of great interest to educators are chapters XII to XVI, in which the author discusses the educability of the dancer, the methods of habit formation, and the duration of habits. As other investigators have found in human learning, so the author finds that for the dancer there is a certain optimum number of repetitions per unit of time,— in this case per day. If fewer repetitions are given, the accomplishment is unduly delayed, while if a larger number be given, the effort seems to be practically wasted. As to the duration of habits, the brightness discrimination gave evidence of persisting for from two to eight weeks, but was seldom perfect after four weeks. None of the habits formed in the color tests lasted over two weeks. Even after all evidence of memory had disappeared a trace evidently remained, just as in human learning, for fewer repititions were required for relearning.

The style of the book is delightfully lucid and entertaining and the contents are of great value not only to students of animal behavior but also to educators and to all who are interested in a comparative study of the learning process.

J. Carleton Bell

*Brooklyn Training School for Teachers.*

LETTERS ON PSYCHOTHERAPEUTICS. *By Professor H. Oppenheim. Translated by* ALEXANDER BRUCE, M.D. Pp. 60. New York, G. E. Stechert & Co , 1907

Between the covers of this little brochure Professor Oppenheim has condensed an amount and variety of psychotherapeutic advice which we do not remember to have seen within an equal space elsewhere. From the letters which in the course of the last few years he has had occasion to send to nervous patients he has selected a number in which the psychotherapeutic point of view is the main consideration. To those of his young colleagues especially, who desire to occupy themselves with psychotherapy,— and that is almost equivalent to saying with treatment in general,— he entertains the hope that his letters will afford some guidance which in some respects may be more welcome to them than many of the writings which deal theoretically with the indications and the nature of psychotherapy. The result is a series of letters of the very greatest practical value; such letters as one could wish to place in the hands of his neurotic patients.

There is not, perhaps, in our day any further necessity to bespeak a respectful hearing for psychotherapy; its importance and utility are admitted by all, so that, in the future the efforts of physicians must undoubtedly turn toward the problems of method. We desire most particularly to know, not that psychotherapy is indicated in a given case, but rather what we are to do and how we are to do it. It is precisely here that Professor Oppenheim comes to our assistance, for he very skilfully instructs us what to say to our perplexed, nervous patients; and this in a manner calculated to carry conviction to their minds and solace to their hearts. If one were to criticise much of the psychotherapeutic literature of this day it might with truth be said that its greatest defect is its lack of concrete definiteness. There is oftentimes rather too much of generalization, with too little of guidance in the manifold difficulties that beset the successful treatment of individual cases. To those who desire to follow a master of individualistic psychotherapeutic correspondence, we commend the brochure of Professor Oppenheim; and, as an example of the kind of help they may expect to receive, we subjoin one of his letters:

"DEAR SIR: —

"The despairing letter which you have sent me to-day shall be answered at once. Certainly I do sympathize with you, for to the sleepless, night is full of distress and day devoid of joy and pleasure; but I know also that this epoch in your life, which to you seems unending, will shortly give place to the happy condition of complete health, and will soon be quite forgotten, even by you. As yet, indeed, you will not listen to my assurance; you cannot imagine that a man who has so completely forgotten how to sleep, on whom hypnotics have begun to exercise a prejudicial effect, can ever return to the normal condition in which a period of waking is, as a matter of course, followed by a period of sleep.

"Now, although I admit that this faint-heartedness is also a symptom of your disease, still at the same time I am convinced that you yourself can be instrumental in mastering it, and with it your illness. You say yourself that during the whole day you cannot help thinking of the coming night and its distress. This thought completely masters you, and you have so entirely given in to it that you are more and more limiting your active habits, and are even neglecting your hobbies, such as riding. But in so doing you are simply barring the path by which you would most rapidly and certainly attain recovery. For the chief requisite for return of your natural sleep is banishment of anxiety about your sleep.

"For that end the following conditions must be complied with. In the first place the time which you now squander in racking your brains and thinking about yourself, must be given to work. This work should be sufficiently interesting; it should at least occupy your attention. It is therefore advisable that it should be varied. Two hours during which your mind is occupied with your business might be followed by an hour of physical work; and even there the uniformity thould not be too constant. You might carve, plane, model, photograph, work in your garden — choose for yourself what pleases you best. But I should specially desire you to ride or motor for an hour during the forenoon or afternoon.

"You think your strength will not be sufficient for all this; but that is a mistake. Your great feelings of weakness,

which form an outcome from your despondency, are not founded upon real weakness. Nor do I intend that you should wear and worry yourself all day long. No; make a long break before, and a small one after the midday meal. Before the evening meal, take a short hour's walk, but have a friend with you who can entertain you. A great deal depends upon the right use of the evening hours. On no account let yourself occupy them with anxious forebodings about the night. But on the other hand, it is not at present wise to take up your mind with too exciting thoughts, as the strong after-impression of feeling and fancy may counteract the tendency to sleep. You must find out for yourself whether a quiet game (cards, halma, chess or patience), the reading of a serious or an amusing book, the perusal of an illustrated paper, or a chat with a friend will be most certain to give you that tranquility of mind through the vestibule of which you will pass into the temple of sleep.

"When you have spent your days and evenings in this way, ideas of anxiety and uneasiness may certainly creep up at times, but they will not have the mastery over you and they will be shaken off so quickly that no soothing thought will be required to wrap you in refreshing slumber. Carry out my instructions then in complete confidence, and I will restore you to the sound health of two years ago.

"Yours faithfully,

O"

JOHN E. DONLEY

# ABSTRACTS

PSYCHO-PHYSICAL INVESTIGATIONS WITH THE GALVANOMETER AND PNEUMOGRAPH IN NORMAL AND INSANE INDIVIDUALS. *By Frederick Peterson and C. G. Jung.* Brain, Part CXVIII. Vol. 30, 1907.

THESE interesting investigations, carried out in the laboratory of clinical psychiatry at Zurich, comprise detailed studies of the psycho-physical reactions and of the association tests in normal individuals and in subjects of dementia praecox. Some of these studies had been already reported by Jung in the Journal of Abnormal Psychology for February, 1907. The chief object of these researches was to ascertain the value of the psycho-physical galvanic reflex as a recorder of psychical changes in normal and in pathological individuals, although parallel with these, pneumographic curves were taken and the associations studied. The apparatus employed was the Marey pneumograph, the kymograph, stop-watch and the Depez-d'Arsonval mirror-galvonometer. Briefly expressed, various physical or psychical stimuli, in particular emotional complexes, bring about certain deviations in the galvonometer and influence the height of the recorded curve. This is the graphic expression of the so-called galvanic reflex. If in connection with these tests there are recorded the changes in the respiratory curve brought about by the emotions and the reaction time and reaction words of the association tests, we have obtained valuable data for an insight into the mental life, or rather the subconscious mental life, of abnormal or insane individuals.

The nervous relations of the sweat glandular system seem to be the chief factor in the production of the electric phenomena ; the emotions thus inducing certain changes in the resistance of the skin to the current. Certain physical causes, such as deep breathing, coughing or increased pressure with the fingers, may also cause fluctuations of the mirror of the galvanometer, but these in the main, are controllable reactions and when once recognized, need not form any source of error. Emotional expressions in normal individuals, such as attention, interest and expectation, cause a rise in the galvanometric curve, and this rise is directly proportional to the intensity of the underlying emotional complex. In all the tests a fixed series of stimuli was

used, such as noises, arithmetical and geographical questions and finally questions having a direct personal significance for the individual. The association test-words were of course variable. For instance, in a normal individual, an unexpected loud noise or the threatened prick of a needle would produce galvanometric and pneumographic curves. Repetition of the stimuli caused a reduction in amplitude of the curves. Both types of curves were measured in height by millimeters. The latent time between the stimulus and the rise of the galvonometer curve is normally from two to five seconds. The pneumographic curves were sometimes profoundly affected by certain stimuli. In a case of acute katatonic stupor, when the patient was called by name, there was observed a marked fluctuation in the pneumograghic curve, while the galvanic curve remained at a straight line, although the latter is far more imtinately connected with subconscious emotional complexes. This pneumographic reaction seemed here to be freely reflex. The two curves illustrating this are well worth studying.

After a detailed study of the curves in normal individuals, a series of tests was made in eleven cases of dementia praecox of various types (paranoiac, hebephrenic and katatonic). For the psychology of dementia praecox the authors accept the theory of inco-ordination or ataxia between affectivity and concepts (Stransky); in other words, an inhibition of the emotional tone. The hebephrenic types showed nothing extraordinary. With the katatonic forms, the latent time was apt to be extraordinarily long, and the galvanometer-waves very slight or absent altogether.

A study of the association-tests was next undertaken and their relation to the pneumographic curves and the galvanic reflex determined. In normal individuals lengthened association time may be taken as an index of a hidden complex, the length of time varying of course with the emotional tone. The arithmetical average of a number of tests in reaction time was parallel with the height of the galvanic curve. Sometimes a strong affect with one stimulus influenced the next succeeding curve in making it unduly high; sometimes a strong emotional tone did not react at the testing time, but at the following indifferent stimulus, as if the emotional tone were postponed. This is really an inhibition of affects, a phenomenon of great importance

in certain hysterical manifestations. This parallelism between reaction time and the height of the galvanometric curve is a proof that long reaction times are emotional phenomena or indices of hidden emotional constellations. If in a reproduction of the test, the curve height or the reaction-word is altered, the latter in the sense of a substitution in an endeavor to conceal the affect, these likewise are emotional phenomena. Now in dementia praecox, the curves and associations do not deviate from the normal, but the most striking fact noted was a lengthening of the reaction time. This lengthening of the reaction time is due to the hindrances of thought in this disease, a blocking of the emotional complex. The paper is an interesting one, presenting as it does some of the newest lines of psychopathological research. Many reproductions of curves and detailed tables are given.

I. H. Coriat

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THE PSYCHOLOGY OF SUDDEN CONVERSION. *By Dr. P. Näcke.* (Reprint from the *Zeitschrift fur Religion Psychologie.*)

In this little pamphlet the writer attempts a psychological analysis of the complex problems of religious conversion. Classifying conversions as "gradual and "sudden" rather than as "volitional" and "surrender types" as Starbuck and James have done, the author points out that in any case of conversion, be it gradual or sudden, two groups of factors must be present. These factors are: first, a suitable soil upon which the conversion can take place, and second, some suitable emotional experience which, acting on the favorable soil, produces the sudden shifting in the individual's point of view.

The more sudden the conversion the greater is the importance of the second factor, the more intensely emotional must be the experience which causes the conversion, while in gradual conversions the second factor, although always present, may be so slight as to be overlooked.

The soil favorable for conversions is the resultant of a large number of factors which determine the individual character and the environment of the person. Some of these factors are endogenous, others are exogenous. Of the endogenous factors the author considers sex, age, condition of the physical health, and the race of the individual. Thus women are naturally more suggestible, more susceptible to all kinds of emotional influences,

less critical and more inclined to be religious. Age is another equally important factor. The instability and heightened emotional tone accompanying the unsettled physiological period of puberty is well recognized. Old age is another period favorable for conversions. The individual has passed the active period of his life, his field of activity and interests are becoming circumscribed; there is increased leisure which is devoted to doubts, religious scruples, and speculations as to the hereafter. In short, it is a period in which the religious traits in one's character become pronounced.

Physical infirmities with their accompanying sufferings, fears, and anxieties, with the time they allow for introspection, with their predisposition to emotional instability are likewise favorable to religious conversion.

But of the utmost importance is the individual character which is determined by a large number of exogenous factors; such as the early influences of the home life and of the school, and later the influences of social relations and occupation. To summarize briefly, we may say that the author points out that all the elements which go to make weakness of character, such as suggestibility, emotionalism, uncritical judgment, etc. are the elements which furnish the favorable soil for conversions.

Given this fertile soil, the next requisite is the occurrence of some suitable, soul stirring event, some experience which should exercise a profound emotional effect on the individual, which should stir up early religious experiences. In cases where the conversion is of dogmatic religious type, that is, where the person is converted to a special confession of faith, as when one becomes Catholic or Protestant, it is indispensable that at the time of the conversion some object or suggestion be at hand to direct the course of the individual to that particular faith and to suggest that this is the only anchor of safety.

So much for the factors, the conditions *sine qua non*, of conversions. As to the mechanism by which conversion takes place, the following explanation is offered.

The sudden emotional experience does not only take up for the time being the field of consciousness, but by association sets into activity long forgotten experiences, which may date back to early childhood. These reawakened memories recur with the emotional tones which they originally evoked. The sum

total of the emotional tone of the experience plus that of the associated memories is so intense, that these newly-awakened ideas come into the full focus of consciousness, the previously existing ideas are submerged and inhibited and we thus get a complete shifting of the point of view of the individual.

Or to state the same thing in physiological language. The intense emotion is accompanied by physiological changes in the circulation, respiration, etc. A change in the body metabolism takes place, the body fluids are altered quantitatively and chemically. These changes in the body fluids produce changes in the activity of the brain, hence the change in the systems of ideas. The circulatory changes accompanying the emotion may bring greater nutrition to association paths long dormant, forgotten experiences with their emotional tones may thus be brought to life again.

From this brief summary it may be seen how closely the writer comes to the views expressed by Prince in his analysis of the state of ecstasy of the Beauchamp case. The main point of difference is this. According to Prince the resuscitated memories come into consciousness during a hypnoidal state, bringing with them their original emotional tones, and while the contents of these memories fall back into oblivion, the emotions they aroused persist after the individual has returned to full consciousness, and these emotions associate themselves with the content of the waking train of thought to which they did not originally belong. Dr. Näcke, on the other hand, maintains that not only is there a return to consciousness of the emotional tone of the dead memories, but that the resuscitated memories themselves, their contents together with their emotional values come into consciousness and displace the existing systems of ideas, thus producing the sudden shift in the point of view. The writer does not present any experimental evidence to substantiate his point of view, and, as he himself says, it is purely theoretical.

One must be struck with the fact that Dr. Näcke in his analysis of the factors which go to make up the fertile soil for conversions, as well as in his explanation of the psychic mechanism of conversions has given us an excellent description of hysteria and the so-called hysterical attack. In fact were the writer to substitute "hysterical accident" for the term conversion, the article could remain as it is, without a change. We

have on the one hand the soil made fertile by heightened suggestibility, emotionalism, and weakened critical powers, a field favorable for disintegrations of all sorts. Then we have the dissociated subconscious states of early experiences. An emotional trauma takes place, and by association the dissociated systems are stirred up into activity and dominate the field of consciousness. What is this but a description of the mechanism of the hysterical accident?

The whole article of Dr. Näcke can be summed up in the statement "Sudden conversion is an hysterical accident."

There are, however, decided objections to this point of view, for after all there is a marked difference between the hysterical attack and the conversion. When the dissociated systems in the case of hysteria are set into activity an attack will result; the dissociated states will dominate the field of consciousness for a shorter or longer period, to disappear again into subconsciousness till by chance they are stimulated again. Nor need the mental state of the hysterical be profoundly modified by this uprushing from time to time, of the subconscious states.

If the mechanism of conversions is the same as that of hysteria why are the two states so different? We have here also an emotional experience which by associative activity arouses memories with their corresponding emotions, so that they become dominant in consciousness and the other existing systems of ideas become submerged. But what keeps up the dominance of these new systems; what is there to prevent the old systems from asserting themselves and reclaiming their position in the focus of consciousness?

The whole difficulty of the question lies in the fact that we have so little experimental observation of cases of conversion upon which to base our theories. There are unquestionably many different types and it is not at all probable that any one explanation will cover all the cases.

H. Linenthal

SWINDLING BY HYPNOSIS (ESCROQUERIE ET HYPNOSE. (*By Bernard Leroy*, Paris; Archives de Psychologie, Vol. VII, No. 26; pp. 138-151.)

THE case which forms the basis of this article is so ununusual in many respects, and the interest attaching to it so exceptional that it abundantly justifies the painstaking study and investigation Leroy has devoted to it.

The patient, Alf., a young woman of nineteen, came to Voisin's clinic at the Salpêtrière in September, 1902. Her story was to the effect that, for several months, she had been the victim of incessant persecution at the hands of a neighbor, one Berthe. This neighbor, it appeared, had the power without contact to create incessant noises and shift about the furniture in Alf.'s apartment. In the patient's own words: "Everything in the house has begun to jump round and make drumming sounds; I don't know what it is that makes such a racket. I hear ventriloquist voices, too."

The patient also affirmed that she was completely under the influence of the neighbor in question. "This person," said she, "has an awful power over me; she has made me carry away all the furniture."

The grandmother of the patient, who accompanied her, confirmed the whole story and alleged that she herself had been victimized and made to sign a receipt for 950 francs, which purported to represent the value of household furnishings taken from her. This sum she had, as a matter of fact, never received, but she had signed the receipt under the terrifying conviction that if she refused, the devil would kill her granddaughter.

Such a history naturally suggested to Leroy that he was dealing with a familial insanity of the persecutory type. Both women were manifestly abnormal, but neither seemed to be actually deluded or subject to hallucinations. There was nothing suggestive from a neurological standpoint in the younger woman's previous history, and her domestic relations were satisfactory. She was possessed of a moderate degree of intelligence, but entertained a belief in a great number of familial superstitions and practiced divination by means of a book. She appears to have refused persistently to believe in "table-tipping," a practice to which her immediate neighbors were passionately addicted. In character she was distrustful and irritable.

The household at Vaujours was composed of her husband, herself and her husband's brother, a boy of seven. In the same town lived the grandmother, a woman still strong in body for one of her years, but feeble in understanding. Most of her time was spent in fortune-telling, another practice in which Alf. is said to have taken no stock whatsoever.

In 1902, Alf.'s father made her a visit at Vaujours and was stunned to find the house practically looted of all its furnishings; the beds being about all that was left. He questioned Alf. and her grandmother, and was informed by them that they had been forced by Berthe, against their will, to sell part of their furniture and to bring the rest to Berthe. Berthe had, it appeared, persuaded them by arguments, into which she craftily introduced the spirits of their departed relatives, and even Satan himself, that if they refused to give up all they possessed, they did so on peril of their lives.

Dissatisfied with this explanation, Alf.'s father decided to bring both her and the grandmother to the Salpêtrière, to have their mental condition looked into; and it is to Leroy's investigations that we owe the following interesting story: In 1901, there came to live in the house with the Alf., the above-mentioned Berthe and her husband, the latter a brother of Alf.'s husband. Berthe, an alcoholic and, as it subsequently developed, a sexual pervert, was looked upon askance by the neighbors generally, while the husband had a prison record for theft.

Alf. held herself aloof from her new neighbors, in fact saw very little of them until June, 1902, when the strange phenomena of which she complained to Leroy began to manifest themselves in her apartment.

That Berthe was at the bottom of all their tribulations was manifest to both women from the start, and it is interesting to note that although Alf. affected deep scorn for such pseudo-supernatural phenomena as table-tipping and fortune-telling, she proved quite as easy a victim as the simple-minded and credulous old grandmother.

To relate in detail the story of Berthe's nefarious methods for hoodwinking and plundering these naïve females would require more space than the reviewer has at his command. Suffice it to say that she evidenced throughout her entire empire over them a knowledge of the weakness of her sex, an histrionic

talent and a versatility, seldom encountered in one of her sort. Of all these methods, including rappings, table-tippings and other sleight-of-hand performances, ventriloquism, etc., not the least interesting was the hypnotic. She was not long in discovering that she could, at will, throw Alf. into somnambulistic states and compel her to execute any commission her own fancy dictated. In such states Alf., with her own money, would procure delicacies for Berthe at the market or would sell her personal belongings and hand over the proceeds.

At the Salpêtrière these somnabulistic states were reproduced by Leroy, and from that time Alf. fully realized how she had been deceived and robbed. This same procedure also cured her of a continual headache from which she had been suffering and enabled her to keep her attention upon her work.

No complaint was entered with the police authorities against Berthe and her worthy husband, on account of the relationship which existed between the last-mentioned and Alf.'s own husband, but the pair were compelled to restore what furniture had not been disposed of. The subsequent history of Alf. is unknown.

J. W. COURTNEY

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IS THE CONDUCTION OF THE NERVE IMPULSE A CHEMICAL OR A PHYSICAL PROCESS? *By S. S. Maxwell.* The Journal of Biological Chemistry, Oct. 1907. Vol. III, No. 5.

The nature of the nerve impulse has often been the subject of investigation, but as yet has received no completely satisfactory solution. The very remarkable resistance of nerves to fatigue and the difficulty of demonstrating satisfactorily the existence of chemical changes as the result of stimulation are well known through the researches of Bowditch, Wedenski and others; and the failure to detect the liberation of heat from a stimulated nerve is equally well known. From such experiments the conclusion has been drawn that the transmission of the nerve impulse is a purely physical, molecular, and not a chemical process. On the other hand it has been argued that all other "life processes" with which we are familiar are associated with some sort of chemical change and that upon purely negative evidence it is not safe to decide that the propagation of the nerve impulse is a unique exception.

The attempts hitherto made to secure positive evidence of chemical change in stimulated nerves have taken in the main four forms: I. The attempt to prove fatigue of the nerve; II. The attempt to demonstrate the presence of the products of chemical change in stimulated nerve; III. The attempt to demonstrate a change of temperature in stimulated nerve; IV. The attempt to prove the necessity of free oxygen to the nerve. None of these experiments, however, nor other work along similar lines, has so far given conclusive evidence as to the chemical or non-chemical nature of the nerve impulse.

In the effect of temperature on reaction velocity van't Hoff and Arrhenius have pointed out a simple and reliable criterion for the discrimination between chemical processes and those which are usually designated physical processes. They have shown that a rise of 10° of temperature increases the velocity of a chemical reaction to two or three times its original rate, while no known physical process is accelerated in anything like so great an amount by a similiar rise of temperature; probably not more than 5 to 15 per cent at most. This principle has been largely used during recent years in biological investigations by Loeb, Snyder, Robertson, Burnett and others.

The experiments of Maxwell were all made upon the pedal nerves of the giant slug *Ariolimax Columbianus*, because of the great length of nerve available and the slowness of the impulse; for the mean rate of impulse is 440 millimeters a second, while it is possible to use a nerve 100 millimeters or more in length. The muscle used consisted of about two or three centimeters of the posterior end of the foot, and the contractions were recorded upon the drum of the kymograph.

The plan usually followed was to take two observations at some convenient temperature; then to change the temperature in the moist chamber 10° and take two observations, after which the nerve was brought back to the original temperature and two more observations were made. The quotient of acceleration was determined by the formula

$$\frac{\textit{Velocity at } T + 10 \textit{ degrees}}{\textit{Velocity at } T \textit{ degrees}}$$

in which $T$ *and* $T + 10$ are absolute temperatures.

As the result of his experiments Maxwell points out the

significant fact that out of 48 quotients, obtained from 43 different nerves, only two are low enough to come within the limit of acceleration of a physical process by a temperature rise of 10°. It seems logical, then, to conclude from the outcome of these experiments that the transmission of the nerve impulse is a chemical process.

Granting this, can we go farther and reach any conclusion as to the particular kind of reaction involved? It would be dangerous to carry speculation very far, but one thing seems to be significant and that is, the relatively low reaction velocity as compared with that of those biological processes which are known to involve oxidations or the giving off of carbon dioxide. The inference may be drawn that the reaction involved in a nerve impulse is not of the nature of an oxidation, an inference made more probable by the fact that the nerve can remain irritable for several hours in lack of oxygen.

The results of this series of experiments may be summarized in a single statement: The temperature coefficient of the velocity of the nerve impulse indicates definitely that the conduction is a chemical process, but probably not of the nature of an oxidation.

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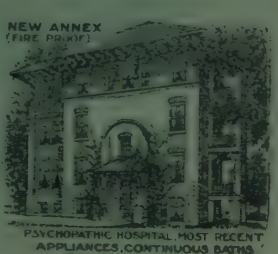

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